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ON TRANSFER AND THE ABILITIES OF MAN[1]

GEORGE A. FERGUSON
McGill University

In 1954 I published a paper in the Journal of this Association on learning and human ability (5). My intent was to draw together these two fields of enquiry, and present a conceptual framework relating each to the other to mutual advantage. In summary the broad features of that theory are these:

(1) The abilities of man, including the reasoning, number, perceptual, and spatial abilities, and whatever is subsumed under intelligence, are attributes of behaviour, which through learning have attained a crude stability or invariance in the adult, and, as they develop in the child, exhibit considerable stability over limited periods of time at particular age levels.

(2) Biological factors in the formation of ability are not excluded. These fix limiting conditions. The implication is that within these boundaries the range of variation in ability attributable to learning is substantial. Thus emphasis is diverted from biological to environmental determination in the formation of ability.

(3) Cultural factors prescribe what shall be learned and at what age; consequently different cultural environments lead to the development of different patterns of ability. Those abilities which are culturally valid, and correlate with numerous performances demanded by the culture, are those that show a marked increment with age.

(4) Abilities emerge through a process of differential transfer and exert their effects differentially in learning situations. Those that transfer and produce their effects at one stage of learning may differ from those at another.

(5) The concept of a general intellective factor, and the high correlations between many psychological tests, are explained by the process of positive transfer, the distinctive abilities which emerge in the adult in any culture being those that tend to facilitate rather than inhibit each other. Learning itself is viewed as a process whereby the abilities of man become differentiated, this process at any stage being facilitated by the abilities already possessed by the individual.

These are a few of the main points of the theory. Criticism of the 1954 paper suggests a need for some refinement and elaboration. Specific

[1]Presidential Address delivered at the Annual Meeting of the Canadian Psychological Association, Ottawa, Ont., June 8, 1956.

Canad. J. Psychol., 1956, 10 (3).

criticism has been directed to the concepts of ability and transfer (1, 4). I propose to elaborate my understanding of these concepts, to review recent experimental evidence on some aspects of the theory, and to comment on a few of its implications and further extensions.

The Meaning of Ability

The term ability is used in a variety of ways. It may refer to measures of performance in any situation, these measures being subject to error in relation to an underlying latent variable, which is presumed to be a continuous monotonic increasing function of the observed measures of performance. The observed measures locate the individual on the latent variable within some range of error. The concept of a latent variable is analogous to that of a parameter in conventional statistical usage. It is a parametric continuum. The latent variable is a necessary construct in that it permits a concept of error, and error in any situation being a departure from a fixed, true, standard, latent, or parametric value. This meaning of ability is operational, and the introduction of a latent variable, although in itself postulational, and not operational, is not incompatible with this meaning but is a necessary logic adjunct to it. All this is in effect an elaboration of Thurstone's statement that "an ability is a trait defined by what an individual can do" (13). The trait here, refers to the latent variable, and the remainder of the statement to measured performance. Scores on psychological tests, measures of performance on learning tasks, and behavioural observations of many kinds are subsumed under this broad meaning of ability. The statement that a learning curve is a description of the change in ability with repetition implies this usage.

Consider now the meaning of ability as a factor in the methodology of factor analysis. This is in effect a derivative of the meaning I have elaborated above. An individual's factor score is a derived measure. It is the weighted additive sum of measures of performance on separate tasks, the weights being obtained by a process of mathematical analysis. Such derived measures are commonly used in physics. For example, the volume of a sphere is given by the formula $V = \frac{4}{3}\pi r^3$, the volume being a function of the radius which is the independent variable. Such functions as this may be called *determined functions*, and the dependent variable is a derived or determined measure. A factor score also implies a latent factor variable, since both the weights and the separate performance measures which determine it are in practice subject to error. The term factor, as distinct from factor score, usually connotes the latent variable. I construe the concept of ability as a factor score to be essentially operational, since it is a determined function of operationally determined variant values.

The term ability is frequently used in a third sense to refer to some attribute of the state of the organism. This state may be vaguely identified with neurophysiological structure and process which is modified by environmental and genetic factors. The state of the organism is presumed to be functionally related, somehow, to observable performance in particular tasks. The persisting notion that differences in intelligence are related to differences in some state of affairs inside people's heads belongs to this class. Likewise the statement that an ability is a learned acquisition, although not entirely learned, implies this usage. The term ability when used in this general way is strictly postulational. No very precise theoretical propositions have as yet been formulated about the nature of ability when construed in this sense, but it may one day be possible to do so. By linking learning and human ability some of the speculations on the neurophysiological bases of learning may have useful implications for ability theory.

No incompatibility exists between the various meanings of the term ability described above. Ability as a latent variable is a necessary logical implication of operationally determined measures of performance. Ability as a factor score is a derived measure. It also implies a latent factor variable. The use of the term ability to refer to some postulated state of the organism is compelled upon us by all those circumstances that require the use of theoretical constructs in science. In the scientific study of ability all these meanings must of necessity co-exist and supplement each other. Of course, different words may be used to distinguish these different meanings, but this is perhaps unnecessary.

In my 1954 paper I used the term ability in all the various ways described above. Likewise I drew a distinction between, on the one hand, performance measures which had attained a crude stability or invariance in relation to learning, and, on the other hand, performance measures which were subject to marked increment or decrement through learning, or its cessation, over relatively short intervals of time. I pointed out that ability was frequently taken to refer in common usage to these more stable measures, and that what was ordinarily meant by intelligence, the perceptual and spatial abilities, reasoning abilities, many of the basic psycho-motor abilities, and the like, seemed to belong to this class. All the above distinctions which were implicit, if not explicit, in my previous work apparently led to certain difficulties in the minds of some of my readers.

The Concept of Transfer

The concept of transfer occupies a crucial position in any theory attempting to relate learning to human ability. For many years I have

been aware of logical difficulties in the commonly accepted concept of transfer. Others, including T. W. Cook (3, 4) have also been aware of this problem. It has long been apparent to Cook, myself, and others that transfer is the more general phenomenon and learning a particular case. In previous theoretical speculation, although pointing to these logical difficulties, I attempted to employ the common interpretation of these terms, but deviated, in the views of some, to an unorthodox position.

I propose now to present a general formulation of the concept of transfer. This formulation generates a number of particular cases. Learning, as conventionally regarded, is one.

Nothing will be said here about the psychological nature of transfer and why or how it occurs. The intent is merely to clarify and restructure the concept, open the way for new forms of experimentation, and provide a conceptual framework within which existing experimental data can be related and reinterpreted.

Transfer, in my view, can best be regarded in terms of the mathematical concept of a function. When two variables are so related that the values of one are dependent on the values of the other, they may be said to be functions of each other. It is customary to distinguish between dependent and independent variables, the value of the dependent variable being dependent on variation in the independent variable. The idea of function is descriptive of change in something with change in something else.

The essence of the idea of transfer, also, is concomitant change, and in the simplest case implies change in performance on one task with change resulting from practice on another.

Consider a simple transfer model comprising measures of performance and amount of practice on two tasks. Denote these by x, y, and t_x, t_y, respectively. Thus we have four variables which may be assumed to be functionally related. The model may, of course, be generalized to include any number of variables, but for the present exposition attention will be directed to the four-variable model only. We may write an expression of the kind $y = \phi(x, t_x, t_y)$, which simply means that performance on one task is some unspecified function of performance on another task, and measures of amount of practice on the two tasks. This expression, and all similar expressions, may be spoken of as transfer functions. Let us now consider particular cases of this general transfer function.

Case I. When the two tasks are considered the same, and y is the same as x for all values of y and x, and t_x is the same as t_y for all values of t_x and t_y, the expression $y = \phi(x, t_x, t_y)$ becomes a two variable function and reduces to $y = \phi(t_y)$ or $x = \phi(t_x)$. These are functions relating measures of performance on a task to measures of amount of practice on that task, and are general expressions for conventional learning curves. In this sense learning is a particular case of transfer and depends on

the identity of x with y, and t_x with t_y. Geometrically this means that the model has been reduced from four to two dimensions. All points fall in a plane or the function is a line, instead of a four dimensional surface.

Case II. When t_x and t_y are very large indeed, and additional practice, or its cessation, produces little effect on performance, a state of crude invariance of performance with practice is attained. Here we may measure performance on x and y for a group of individuals and study the relationship between the two sets of measures. I contend that the correlations between many tests, commonly called tests of ability, are in effect correlations between performances which through learning have attained a crude invariance. If this is so it follows in many cases that the correlations between tests of ability can be viewed as particular cases of the more general transfer function. Thus we have a useful linkage between learning and ability.

Case III. When t_y is a constant, that is, where no practice is allowed on the y task, we have a situation involving three variables, y, x, and t_x. We may consider the function $y = \phi(x, t_x)$, or $y = \phi(t_x)$, or $y = \phi(x)$. This latter expression is descriptive of change in y, practice being held constant, with change in x resulting from practice. In effect it implicitly defines the amount of practice in terms of change in performance and not in terms of an independent measure, as, for example, the number of trials. This particular function is of some interest in that it suggests modifications in current thinking on transfer and opens up new lines of experimentation.

The simple transfer function $y = \phi(x)$, when stated in this form, generates, of course, an indefinite number of particular functions. Some of these are deserving of comment. First, consider the case where no change occurs in y with change in x. Here no transfer in fact occurs. Second, consider the case where a constant increment occurs in y with increase in x; the curve describing the relationship is linear and has a positive slope within fixed ranges of the variables. This is a case of linear positive transfer. Similarly the case of linear negative transfer can be recognized. Third, many cases of non-linear transfer functions can be identified. For example, y may show an increase with x over some range of x and no increase or a decrement over some other range. This means that, in the learning of a task, positive transfer may occur at one stage of learning, and no transfer or negative transfer at another. All non-linear cases of this kind may be spoken of as examples of differential transfer, the rate of transfer differing at different stages of learning. Fourth, consider a case where y continues to change after no improvement in x is observed. If examples of this type could be demonstrated experimentally, this would argue for a modification of behaviour through overlearning, despite the absence of any observable change in performance on the learning task itself. This in turn would suggest some continuing modification in the neurophysiological state of the organism during overlearning.

All the above can be greatly elaborated and stated in more precise mathematical form. In general the introduction of the idea of a transfer

function argues very simply for the use of the concept of continuous covariation in the study of transfer, and the discarding of discrete concepts. Many of the existing concepts in the field of transfer are, in effect, discrete, such as the conventional distinction between positive and negative transfer. The concern of the experimentalist has frequently been with the demonstration of the presence or absence of certain effects, rather than with the mapping of descriptive continuous functional dependencies. The point has been reached in our experimental exploration where concepts of a more general, and consequently more powerful, nature are required. The ideas on transfer I have presented here are a small step in this direction.

The concept of a transfer function has important implications for experimental design. It is possible in practice to design experiments which embody the ideas on transfer presented here. Such designs are in certain cases general extensions of the existing designs used in experiments on transfer. A variety of new problems will of course present themselves. Unfortunately time does not allow a treatment of experimental design here.

The above discussion points to the following very general formulation. At any given point in time the organism may be said to be in a particular state. The concept of the state of a system is of importance in physics. It has a role in psychology also. This state undergoes continuous change because of a large number of circumstances both inside and outside the organism. One set of factors leading to a change in state is the behaviour of the organism in response to specific environmental circumstances, e.g. the performance of a task. Any change of state leads, theoretically, to changes in an indefinitely large number of other possible forms of performance. Any covariation which can be identified between any two or more forms of performance is conceptualized as a transfer function. Certain aspects of the state of the organism attain a crude stability or invariance and are less susceptible than others to modification through continuing behaviour and other factors over limited ranges of time. These, postulationally, are invariants of the state of the organism, which in turn are functionally related to certain invariants in particular observable behaviours. What we conventionally regard as the abilities of man are among these invariants. The whole process of growth and development is directed towards the reduction of uncertainty in the behaviour of the organism, and to the establishment of these invariants. Thus behaviour becomes organized, or structured, and to some extent predictable. The most noteworthy examples of the prediction of human behaviour in psychology are made through a knowledge of these invariants. Although concern here is with the study of ability, what we conventionally regard

as personality, as distinct from ability, can be similarly conceptualized. Characteristics of personality, attitudes, and the like can be viewed as attributes of behaviour which have attained some stability through a lengthy learning process. The discovery of these invariants in the behaviour of man is one of the primary objects of psychological endeavour.

Recent Experimental Evidence

Supporting evidence for the theory relating learning to human ability is substantial, and is scattered broadly through both the psychological and the cultural anthropological literature, and elsewhere. I do not propose to attempt to collate or review this evidence here, but shall confine my remarks to two recent series of studies: those by Edwin A. Fleishman (7, 8, 9) of the Air Force Personnel and Training Research Centre at Lackland Air Force Base, and those by Dr. Alastair Burnett (2) of the Hospital for Mental and Nervous Diseases, St. John's, Newfoundland.

The design of the Fleishman studies involved the administration of learning tasks to groups of subjects and the recording of measures of performance at a number of stages of practice. In one study the learning task was a Complex Coordination Test and in another a Discrimination Reaction Time Test. In both tasks subjects were required to make motor adjustments or manual responses to visual stimulus patterns. In addition to the learning tasks, a variety of tests were administered to the subjects involving spatial, verbal, perceptual, psychomotor, and other abilities. Intercorrelations were obtained between all measures including measures of performance on the learning task at different stages of practice. The correlations were analyzed using standard factorial methods. One study involved 197 and the other 264 subjects.

These studies show conclusively that substantial and systematic changes occur in the factor structure of the learning task as practice continues. The abilities involved at one stage of learning differ from the abilities involved at another stage. Thus conclusive experimental evidence exists to support the hypothesis of differential transfer. Further these studies suggest that certain non-motor abilities, e.g. spatial and verbal, have a greater involvement in the earlier stages of learning of a psychomotor task than in the later stages, whereas the opposite applies to certain motor abilities, e.g. reaction time and rate of movement. This is in line with the notion that cognitive abilities play a more important role in the earlier stages of the learning of a motor task than in the later stages, when performance becomes organized in the form of a habituated psychomotor response pattern. Generalizations on this point are of course precarious because the distinction between cognitive and motor abilities is arbitrary,

and because much will depend on the nature of the particular learning task.

Some of Fleishman's results raise important questions on the nature of adult learning. His studies show that factors specific to the tasks themselves increase in importance as practice continues, suggesting the formation of specific or "within-task" abilities, not involving previously established abilities. Acceptance of this finding might require some modification of the theoretical position that adult learning is in large measure a process involving the reorganization or integration of that which has been previously learned. It would mean also in practice that our capacity to predict human behaviour would be limited, and would ever remain in some large measure indeterminate. These findings are not acceptable as they stand, and fortunately there are avenues of escape from the experimental data. The most plausible of these is that the tendency for the specific "within-test" variance to increase with practice is a result of the selection of reference variables of ability, and that with the inclusion of new and different reference variables a higher proportion of this variance at the more advanced stages of learning can be brought under control. If adult learning is a reorganizing or integrating process, involving different ability patterns at different stages, it seems reasonable to suggest that individuals may exhibit differing facilities in organizing or integrating their abilities to cope with a new task. Thus we are led to hypothesize certain basic integration abilities, which may have an important role to play in adult learning. Considerable information exists in the literature of factor analysis (6, 10) on the existence of such integration abilities, but this information has not, hitherto, been brought to bear on the problem of adult learning. Fleishman is currently engaged in a comprehensive investigation of the involvement of certain integration abilities in performance at different stages of practice on psychomotor tasks. It is hoped that these studies will show that a higher proportion of the specific within-task variance can be brought under control. If experimentation demonstrates that this is not so, then we may be required to revise existing theories of adult learning and make some radical changes in our current concepts of predicting human behaviour, or evolve another line of attack.

In passing I should mention that the Fleishman experiments provide the empirical buttressing for a number of important hypotheses in my theory which in turn provides a theoretical basis for his experiments. Although I had previously discussed the design of experiments of this type, their conduct, which is very exacting and laborious, lies well beyond the range of the facilities of most university investigators.

Dr. Burnett's work carried on at McGill and at the Hospital for Mental and Nervous Diseases in Newfoundland was directed towards the assessment of the abilities of individuals living in relatively isolated outport communities. Burnett's work shows conclusively that the pattern of ability of children reared in relatively isolated outport communities differs markedly from that of children reared in urban centres. In the isolated Newfoundland environment certain perceptual and motor abilities are developed to a high level, whereas verbal and reasoning abilities are less well developed. A retardation in abstract thinking and concept formation seems to occur. These findings go well beyond the simple observation that tests which may be appropriate in one culture may be inappropriate in another, with its implication for the development of culture-free tests. Everything we know suggests that different environmental demands lead to the development of different ability patterns. The concept of a culture-free test is a misconception because the abilities of man are themselves not culture-free. The extensive body of literature on the abilities of individuals reared in cultures markedly different from our own in general supports this conclusion, and adds substantial evidence for the role of learning in the formation of abilities.

Some Implications of the Theory

The implications of the theory for contemporary theoretical and experimental work in psychology are numerous. I propose to comment on a few of these implications here.

One implication is that some of the data obtained from the study of human ability using an individual difference approach, may with reinterpretation have relevance for learning theory, and that the methodology of ability studies may be brought to bear on learning problems. The substantial body of data, summarized in my 1954 paper, on the relationship between psychological test performance and age in different cultural groups has, for example, direct relevance for learning. Fleishman's work is an excellent example of the application of a methodology extensively used in the ability field to problems which have, among others, important implications for any theory of learning in the adult. Quite recently I have devoted some thought to the problem of intra-individual variability in ability patterns, or profile analysis, and have formulated hypotheses with implications for learning theory which can be investigated by the study of such profiles. This work bears on the organization or integration of abilities through learning. It is still at a rudimentary stage and further comment on it would be unwarranted.

Learning theorists have frequently preoccupied themselves with some-

what involved theories to explain the nature of the learning process itself. They have, of necessity, tended to restrict themselves to a range of learning tasks of low-order generality. No satisfactory methodology has emerged for describing particular learning tasks, or indicating how one task differs from another, other than by a process of simple inspection. The ability theorists have, however, developed descriptive classificatory systems, which, regardless of their many faults, do have some degree of generality in relation to many forms of human behaviour. Since abilities are clearly involved in the learning process, it follows that particular learning tasks can, at different stages of learning, be described in terms of particular ability patterns. We have, then, a method for describing particular learning tasks and differentiating them one from another. Thus it may be possible to make progress in removing learning theory from the context of particular tasks. Students of human ability have long been aware that, just because two tasks looked on inspection very much the same or different, they were not of necessity the same or different in terms of the behavioural responses which they elicited. In consequence it has been the practice, in the ability field, to describe the task situation or the stimulus in terms of the response. This approach underlies the whole of factor analysis. The description of the stimulus in terms of the response is an approach which has apparently not found favour in the learning field where it undoubtedly has application.

A further implication of the theory resides in its emphasis on environmental factors in the formation of ability. In my early training in psychology I was led to believe that man's abilities were irrevocably fixed and rendered unchangeable by biological endowment. This position is no longer tenable. Although it is conceded that biological factors fix certain boundaries, all the evidence seems to suggest that the range of variation that results from learning is, indeed, very great. If this is so, it immediately raises questions of value and social responsibility. It means that a society, through control of the environment and the educative process, can in some considerable degree determine the patterns of ability which emerge in its members. This has long been known to apply in the formation of attitudes. Its extension to the field of man's abilities broadens the area of social responsibility which the psychologist, through his knowledge of human behaviour, may one day be required to face.

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EFFECT OF GLUTAMIC ACID ON THE LEARNING ABILITY OF BRIGHT AND DULL RATS: I. ADMINISTRATION DURING INFANCY[1]

K. R. HUGHES AND JOHN P. ZUBEK
University of Manitoba

IN 1944 Zimmerman and Ross (27), working at Columbia University, reported that the feeding of glutamic acid to young rats resulted in a considerable improvement in maze-learning ability. Later another group of Columbia workers (2) reported beneficial effects on the performance of rats in a complex reasoning problem. Extension of this work to mentally retarded children has suggested that glutamic acid may increase the IQ as measured by standard intelligence tests (1, 4, 8, 16, 20, 25). A number of negative studies have also been reported, however (5, 6, 10, 13, 15, 24).

Since the early Columbia studies, every single attempt to verify the positive animal findings has failed (3, 7, 11, 12, 17, 21, 23). Perhaps the most crucial of these animal studies was carried out by Stellar and McElroy (21), who duplicated every possible feature of the original Zimmerman and Ross experiment (except strain) and still obtained negative results. As a result of such findings the original enthusiasm about the possibilities of glutamic acid has almost disappeared, and little or no investigation, at either the animal or the human level, is now being done.

One important variable, which has been overlooked in the animal research on the effects of glutamic acid, is that of differences in learning ability from one strain to another. Examination of the learning scores of the Zimmerman and Ross control animals shows that they made many more errors than did the control animals of Stellar and McElroy, though tested on an identical maze and under similar conditions (36.5 *vs.* 13.4 errors). It would thus appear that the former group of workers was using a much duller strain of animals than was the latter. If this is the case, then differences between strains in initial learning ability may be the cause of the conflicting data, for it is conceivable that glutamic acid may facilitate the learning ability of dull animals but have no effect on bright ones. An obvious test of this hypothesis would be to obtain strains of bright and of dull rats and determine whether glutamic acid has a differential effect on the two groups, improving the learning ability of

[1]This research was supported by a grant-in-aid from the National Research Council of Canada.

the dull animals, but having no effect on the bright ones. Since such strains were available in the colony at Manitoba, it was decided to use them in an experimental test of the hypothesis.

Experiment I

The first experiment was largely an exploratory study to determine whether glutamic acid has any effect on the maze-learning ability of a strain of dull rats.

Subjects. Thirty-one naïve, hooded rats of the McGill dull strain (F_{10}) were used. These animals had been selectively bred for dullness on the Hebb-Williams maze. They were divided into an experimental group containing 18 rats (6 males, 12 females) and a control group containing 13 rats (7 males, 6 females).[2]

Apparatus. The 12 problems of the Hebb-Williams closed-field maze were used in the manner described by Rabinovitch and Rosvold (18).

Procedure. The 31 animals of the dull strain were weaned at 25 days of age, and both the experimental and the control groups were placed on a normal colony diet (Fox chow pellets). In addition to this food, the experimental animals were given a daily supplement of 5 grams of wet mash, containing 200 mg. of monosodium glutamate, while the controls received the 5 grams of wet mash, but without any added glutamate. This feeding procedure was continued for 40 days. At 65 days of age the food supplement was discontinued, and all animals began their adaptation and preliminary sessions on the Hebb-Williams maze. The 12 problems were then presented, two problems per day, approximately 9 hours apart. Eight trials were given on each problem. The learning score of each animal was the total number of error zones entered in the 12 maze problems. Time scores were also kept. Time was recorded from the moment the animal passed through the entrance door until it took its first bite of food.

Results

The mean error and time scores for the dull experimental and control groups are given in Table I. It can be seen that the dull experimentals made fewer errors than did the controls. This difference is statistically significant ($t = 3.02$, $p < .01$). The dull experimentals also showed better time scores than the controls. This difference is also significant ($t = 3.17$, $p < .01$).

TABLE I

Mean Error and Time Scores of Dull Experimental and Control Rats in Experiment I

	Dull experimental	Dull control
Mean errors	107.6	144.9
Mean time (secs.)	407.2	541.1

[2]The uneven sex matching was due to accidental loss of several cages of animals. There were no significant differences between the performances of males and females in any of the groups.

Experiment II

Since these results were the first positive ones to be obtained since the early Columbia studies it was decided to repeat the entire procedure with another group of dull animals, in order to be certain that glutamic acid really has a beneficial effect on maze-learning ability. A bright strain was also used in this experiment to see whether it would fail to show improvement.

Subjects. Twenty-four naïve, hooded rats of the McGill dull strain (F_{11}) were used. These were divided at weaning (25 days) into an experimental group containing 13 rats (7 males, 6 females) and a control group containing 11 rats (5 males, 6 females). A group of 19 hooded rats of the McGill bright strain (F_{11}) was also used. These were divided at weaning into an experimental group containing 8 animals (6 males, 2 females) and a control group of 11 animals (5 males, 6 females).

Procedure. The dull experimentals and bright experimentals received a daily supplement of 200 mg. of monosodium glutamate (in 5 grams of wet mash) for 40 days, while the dull and bright controls received a placebo supplement (plain wet mash). Following the adaptation and preliminary sessions all four groups of animals were tested on the 12 problems of the Hebb-Williams test at the rate of two problems a day, approximately 9 hours apart.

TABLE II

Mean Error and Time Scores of Dull and Bright, Experimental and Control Rats in Experiment II

	Dull		Bright	
	Experimental	Control	Experimental	Control
Mean errors	127.5	164.0	116.5	117.0
Mean Time (secs.)	601.8	850.0	372.0	391.5

Results

Table II summarizes the error and time scores of the dull and bright, experimental and control groups. From the table we see that the dull experimentals again made significantly fewer errors ($t = 2.16$, $p > .01 < 0.5$) and took significantly less time ($t = 3.05$, $p < .01$) in learning the maze problems than did the dull control animals. However, glutamic acid does not seem to have had any effect on the bright strain, the error scores of the experimental and control groups being almost identical. The time scores show the bright experimentals as slightly superior, but the difference is not statistically significant ($t = 0.99$, $p > .10$).

Discussion

The results of the two experiments indicate that glutamic acid does have a beneficial effect on dull rats and can increase their learning ability

considerably. On the other hand, this substance does not influence the learning ability of bright animals. It thus appears that a possible explanation for the conflicting reports in the literature on animals lies in the overlooked variable of differences between strains in initial learning ability. The workers at Columbia may, for some reason, have possessed an animal strain that was much duller than the strains in other laboratories. These duller animals would, in the light of the present results, show improvement in learning ability, while the brighter ones used by other investigators, e.g. Stellar and McElroy (21), would not. Both Stellar and McElroy and Porter and Griffin (17), in reporting their negative studies, suggested that the effects of glutamic acid might be specific to the Sherman strain kept at Columbia University. This suggestion, however, was never followed up, and the question of "strain specific effects" remained unanswered.

These findings on the differential action of glutamic acid are in line with the clinical data. In no case has glutamic acid benefited human subjects of normal or above normal intellectual ability (1, 25). The positive results have always been obtained with a mentally retarded sample. It is not surprising, therefore, that when an analgous selection of animal subjects was made the results should turn out to be the same.

What is the mechanism underlying this improvement in learning ability? One possibility is that the acid mediates its effect simply by alleviating some pre-existing dietary deficiency of glutamate. This, however, seems not to be the case. First, rats are known to be able to synthesize glutamic acid in quantities sufficient to meet their bodily requirements. Secondly, Porter and Griffin (17) have demonstrated that animals raised on diets deficient in glutamic acid show no improvement in learning ability following excess glutamate supplementation. A much more likely hypothesis is offered by Zimmerman *et al.* (26), who suggest that the improvement in learning ability may be due to the facilitatory effect of glutamic acid upon certain metabolic processes underlying neural activity. In support of this view they cite Nachmansohn *et al.* (14), who have shown that glutamic acid is important in the synthesis of acetylcholine, a chemical substance necessary for the production of various electrical changes occurring during neural transmission. Nachmansohn reports that the rate of acetylcholine formation could be increased four to five times by adding glutamic acid to dialysed extracts of rat brain. More recent work (22) has shown that the concentration of this acid in the brain is disproportionately high, as compared with the concentration of other amino acids or with its concentration in other body tissues. Furthermore, of all the amino acids, glutamic acid alone is capable of serving as the respiratory substrate of the brain in lieu of

glucose. This further points to the involvement of glutamic acid in neural function. Finally, Sauri (19), using rats, found that the acid exerts its main action on the cerebral cortex, lowering the threshold of excitability. All this clearly points to the importance of glutamic acid in cerebral metabolism.

Zimmerman's suggestion still does not account for the differential effect of glutamic acid on the learning ability of the two strains of animals. However, if we assume that the cerebral metabolism of the dull rats is defective in some way, while that of the bright rats is normal, then glutamic acid might facilitate or improve the defective cerebral metabolism of the dull animals, while having no particular effect on the normal metabolism of the bright ones. A relationship between cerebral metabolism and mental functioning has been demonstrated by Himwich and Fazekas (9). In a careful study of tissue preparations from the brains of mentally retarded persons, they were able to show that these tissues were incapable of utilizing normal amounts of oxygen and carbohydrates. For example, in cases of mongolian idiocy and phenylpyruvic oligophrenia, the brain removed much less than the normal amounts of oxygen and glucose from a given volume of blood passing through it. In other words, the cerebral metabolism of these mentally retarded patients was defective. In the light of this work it would be of importance to know something about the cerebral metabolic state of the two animal strains, and especially of the glutamate-fed dulls. Since the necessary biochemical techniques are already available, this should not present too much of a problem.

The experimental findings of this study raise a number of questions. How permanent is the improved learning ability? Will it continue without further supplements of glutamic acid, or must the acid diet be continued for the improvement to last? Will dosages of acid greater than 200 mg. per day still further improve learning ability? What is the role of age? Will administration of glutamic acid to adult animals also produce improvement, or must it be given during infancy? Some of these questions we hope to answer in the near future.

Summary

The purpose of this experiment was to study the effect of glutamic acid on the maze-learning ability of bright and dull rats (McGill strain).

Each strain of animals was divided into an experimental and control group at the time of weaning (25 days). The dull experimentals and bright experimentals received a daily supplement of 200 mg. of monosodium glutamate (in 5 grams of wet mash) for 40 days, while the dull and bright controls received a placebo supplement. Following the usual

adaptation and preliminary sessions, the animals were tested on the 12 problems of the Hebb-Williams maze at the rate of two problems a day.

The results indicate that glutamic acid can increase the learning ability of dull rats considerably. It does not, on the other hand, affect the learning ability of bright rats.

Possible mechanisms underlying the improvement are discussed.

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STIMULATION-MATURATION PREDICTION OF DISTRIBUTION PHENOMENA IN COMPENSATORY PURSUIT[1]

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THE maturation hypothesis of learning is rooted in the writings of Wheeler (10) and of Wheeler and Perkins (11). It was first stated in specific enough form to be subject to experimental check by Doré and Hilgard (4). The hypothesis has two aspects, which, as phrased by Doré and Hilgard, are as follows:—

1. *Age maturation.* "The capacity of an organism to improve with practice is a function of its age-level of maturation" (4, p. 246).

2. *Stimulation-induced maturation.* "At any given age-maturation level the optimum rate of stimulation to secure improvement in a given function depends upon the normal rate of stimulation-induced maturation in that function" (4, p. 246).

The first part, needless to say, is beyond doubt; the correlation of learning efficiency with the performer's maturation level, i.e. age, is almost a truism. But the second part, which postulates some kind of growth process following the stimulation from learning activity, holds a controversial position. It is with the latter that the present paper is concerned.

Much akin to what Snoddy (9) calls "primary growth," stimulation-induced maturation is a function of time. It is time rather than practice that matters. This principle, however, is said to hold good within certain limits which vary according to the nature of the task. The effect of time cannot be fully realized when the stimulation is insufficient, and, conversely, overstimulation may impede time's maturational function.

The hypothesis of stimulation-induced maturation leads to certain clear-cut inferences relating to the problem of massed vs. distributed learning. One of these inferences, as put by Doré and Hilgard, is: "At any given age level of maturation, distributed stimulation in a specific function will give rise to an improvement in that function at a rate which is related more closely to the time elapsing from the beginning of practice than to the total amount of practice" (4, p. 253).

Doré and Hilgard carried out an experiment with the Koerth pursuit rotor to test this inference. Four groups of subjects, 25 in each, were

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given 7, 13, 22, and 31 one-minute trials, respectively, in a total period of 43 minutes. At the 41st minute the latter three groups scored essentially alike in spite of wide differences in the amount of practice, suggesting that time, rather than mere practice, might be the primary determinant of the level of performance. The group which spent only eight minutes in actual practice fared poorly. The explanation of this, according to the stimulation-maturation hypothesis, would be that stimulation was insufficient to induce the maximum growth that the given time could yield. Results in an experiment reported by Hilgard and Smith (6), also with the pursuit rotor, agree with the finding of Doré and Hilgard, and are also consistent with the view that greater stimulation than the maturation process can absorb in a given time is not only useless but even detrimental.

Contradictory findings have been reported by Wright and Taylor (12). In their experiment five groups of 16 subjects each were trained for a total period of 52 minutes, in anticipating a serial list of 18 nonsense syllables exposed at the rate of one every three seconds. One group practised continuously; the other four groups were allowed rest intervals of 1¼, 2, 3½ and 8 minutes between repetitions of the list. The results, showing that performance conforms more nearly to the fraction of the total time spent in actual practice than to the total time elapsed from the beginning of practice, were radically different from what Hilgard and his associates had found. At the 34th minute after the beginning of practice—chosen because all groups had a trial at this point—the group practising continuously made a mean of 13.44 correct anticipations. The groups given intertrial rests of 1¼, 2, 3½, and 8 minutes scored 10.75, 9.12, 6.37, and 3.36. These scores should have been more nearly alike in magnitude if the maturational hypothesis held.

The experiment reported in this paper was designed to test the hypothesis of stimulation-induced maturation in compensatory pursuit. Wright and Taylor used verbal material, whereas the experiments of Hilgard and his associates involved the pursuit rotor. If the negative outcome of the experiment of Wright and Taylor was primarily due to difference of material, we might expect that the present experiment, on a task similar to that used by Doré and Hilgard and Hilgard and Smith, would give results more nearly consistent with their findings and the stimulation-maturation hypothesis.

Method

The apparatus consisted of three units of the Airplane Control Test obtained on loan from the Lackland Air Force Base, San Antonio, Texas. This test consists of a miniature airplane so controlled by cams and pulleys that it moves in a specific three-dimensional pattern, constantly changing its pitch, bank, and turn. The *S*'s task, which can be best described as compensatory pursuit, is to counter this movement by

manœuvring a stick and rudder pedals in a manner that keeps the airplane pointed straight ahead and in a level flight position. A small light situated underneath the airplane guides the S, glowing red whenever the airplane is in correct position, thus giving knowledge of performance. The total time during which the airplane is kept in correct position during each trial is the measure of performance, which is recorded in units of .001 of a minute by electric timers. As a trial lasts for one minute, the total possible score is 1000.

Rest interval between 1-minute trials was varied in five different ways for five groups of 21 Ss each, referred to hereafter as A, B, C, D, and E. Group A practised continuously for 45 min. Groups B, C, D, and E were tested for 44½, 45, 43, and 43 min., with intertrial rests of ½, 1, 2½, and 5 min., respectively. The five groups practised for 45, 30, 23, 13, and 8 trials. These facts can be shown in tabulated form as follows:

Group	Duration of intertrial rest (minutes)	Number of trials	Total experimental time (minutes)
A	0	45	45
B	½	30	44½
C	1	23	45
D	2½	13	43
E	5	8	43

The experiment was carried out at Central High School in Louisville.[3] The Ss, 105 boys in all, were 11th and 12th graders. The purpose of confining the experiment to two grades was to hold the factor of age maturation as nearly constant as possible.

As there were three test units available, three boys were run at a time, screens being interposed to keep the co-performers from watching each other. Thirty-five such groups of three boys each were randomly assigned to the five experimental conditions, seven groups to each condition.

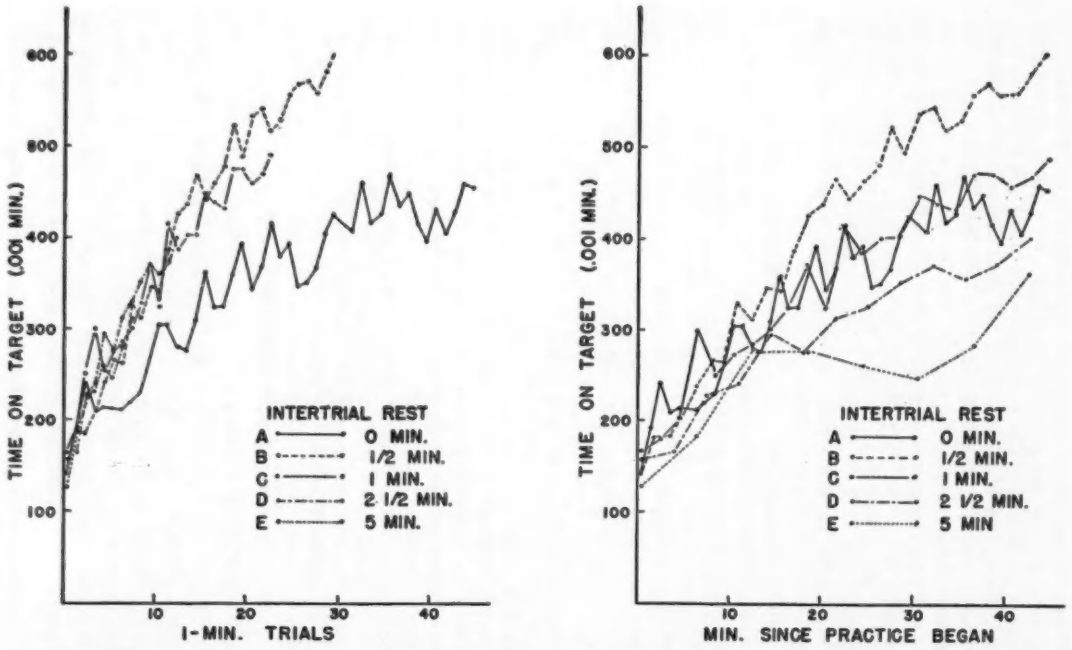
The task was explained with demonstration to the Ss. Before starting, the *E* made sure that all three Ss understood the procedure. During rest intervals the Ss were instructed to talk among themselves, think about matters unrelated to the experiment, or look through the popular magazines provided.

Results and Discussion

1. *Massed vs. Distributed Practice*

The results of this experiment show the familiar advantage in performance of distributed over massed practice, but do not reveal any steady increase in performance level with increasing duration of interpolated rest as observed in many experiments with perceptual-motor tasks (e.g., **1, 2, 3, 5, 7**). The performance curves given in Figure 1A were obtained by plotting the mean time-on-target scores of each of the five groups as a

[3]Without the assistance of Mr. Atwood Wilson, Mrs. Betty Douglas, and many teachers, this research could never have been carried out. The strong support given Air Force research projects by Mr. Morton Walker, member of the Louisville school board, and by Dr. Omer Carmichael and Mr. W. F. Coslow, Superintendent and Assistant Superintendent of the Louisville Public Schools, is gratefully acknowledged.



A. SCORES PLOTTED AS A FUNCTION OF AMOUNT OF PRACTICE.

B. SCORES PLOTTED AS A FUNCTION OF TOTAL ELAPSED TIME SINCE START OF PRACTICE.

FIGURE 1. Performance as a function of temporal distribution of practice.

function of practice trials.[4] Although the unequal length of these performance curves does not allow a complete comparison, it is obvious that the curves for Groups B, C, D, and E are approximately of the same height, whereas the curve for continuous practice (Group A) is consistently inferior to all of them.

The statistical tests of significance summarized in Tables I and II lend support to the above observation. Table I shows the outcome of the test by the method of analysis of variance for significance of differences among the five groups at Trials 1 and 8; among A, B, C, and D at Trial 13; among A, B, and C at Trial 23; and between A and B at Trial 30. The small F ratio at Trial 1 lends support to the assumption that the five groups were initially of comparable learning ability. The F ratios corresponding to Trials 8, 13, and 23 are also not large. Only at Trial 30 was there a difference significant at the 1 per cent level. Evidently, similarity of the performance levels of Groups B, C, D, and E outweighed the difference of Group A from the rest. The observation that Group A's performance is inferior to that of the others is borne out by t tests of the differences of Group A from Group B at Trials 8, 13, 23, and 30; from Group C at Trials 8, 13, and 23; from Group D at Trials 8 and 13, and from Group E at Trial 8. In all ten comparisons, the difference was in the expected direction and was statistically significant at or beyond the 5 per cent level (single-tail test).

The fact that there is an optimum distribution beyond which increase of interpolated rest does not bring any further improvement (or even

TABLE I

RESULTS OF TESTS FOR SIGNIFICANCE OF DIFFERENCES AMONG THE GROUPS AT THE 1ST, 8TH, 13TH, AND 23RD TRIALS

Ordinal number of trial	Group mean scores					Between-group variance estimate	Error variance estimate	Degrees of freedom	F
	A	B	C	D	E				
1	141.1	141.8	161.8	159.5	128.4	4102	9349	4 and 100	—
8	220.4	331.2	301.0	327.9	313.5	43365	26837	4 and 100	1.67
13	280.1	424.9	385.6	401.0		85853	39073	3 and 80	2.20*
23	416.8	517.3	488.9			56431	34629	2 and 60	1.63
30	427.0	607.6				342725	36563	1 and 40	9.89**

*Significant at 10 per cent level of confidence.

**Significant at 1 per cent level of confidence.

[4] A series of tables giving all individual raw scores and all group means has been deposited with the American Documentation Institute. Order Document No. 4865 from American Documentation Institute, 1719 N. St. N.W., Washington 6, D.C., remitting $1.25 for 35 mm. microfilm or $1.25 for 6 by 8 in. photocopies.

worsens the achievement) has been noted by a number of authors. Ammons (2, 3) found that Ss given intertrial rests of 5 and 12 minutes during acquisition of the rotary pursuit habit performed less well than Ss given 2-minute rests. Snoddy (9) had groups of Ss practise mirror tracing with rest-intervals of 0, 30 and 60 sec., and 24 hr. He found 30 sec. to be as superior to continuous practice as any other distribution, and this finding agrees well with results in the present experiment (Section 2 below). The near-optimum intertrial rests observed by Kientzle (7) for inverted alphabet printing and by Wright and Taylor (12) for serial learning of nonsense syllables are about 45 and 75 sec.

These findings suggest that the limit of effectiveness of added rest, which varies from experiment to experiment, may well be a function of task. Evidence supporting this position is found in Lorge's report (8) of an investigation in which distribution effects in mirror reading, code substitution, and nonsense syllable learning were compared and gave different results.

2. *Test of Maturation Hypothesis*

The maturation hypothesis, as explained in our statement of problem, predicts that learning will be a function of total elapsed time rather than of mere practice time; hence, if curves are plotted in terms of time elapsed since the inception of practice, they should look alike.

One or two of the curves may show poor achievement due to either overstimulation or understimulation, but, if the rates of distribution do not differ widely, the majority should follow approximately the same course. Curves plotted as a function of elapsed time for the present five groups are shown in Figure 1B. The curves cross and recross in the initial stage, but the differences become marked with time.

TABLE II

ANALYSIS OF VARIANCE OF THE SCORES OF THE FIVE GROUPS AT THE 43RD MINUTE OF ELAPSED TIME

Group	Ordinal number of trial at the 43rd minute of elapsed time	Mean score	Between-group variance estimate	Within-group variance estimate	*F*
A	43	430.6			
B	29	581.6			
C	22	468.4	201888	39167	5.15*
D	13	401.0			
E	8	313.5			

*Significant at beyond the 1 per cent level with 4 and 100 degrees of freedom.

Analysis of variance of the scores of the five groups at the 43rd minute of elapsed time yields an F ratio of 5.15 (Table II) which is significant for 4 and 100 degrees of freedom at the 1 per cent level. The scores for the 43rd minute were chosen for comparison, because at this point in elapsed time all the groups had a trial: Trials 43, 29, 22, 13, and 8 for Groups A, B, C, D, and E, respectively.

Differences at Minute 43 between each of the two extreme groups (B and E) and the remaining groups are significant at or beyond the 10 per cent level of confidence in every case. The poor performance of Group E (5-minute intertrial rests) can be regarded as consistent with the stimulation-maturation hypothesis on the grounds that this group was understimulated. However, the superior performance by Group B (½-minute rests) cannot be accounted for so easily. We might say that, relative to Group B, Group A (continuous practice) was overstimulated, and Groups C and D (1-minute and 2½-minute intertrial rests) were progressively more understimulated. The actual means (Table II) are neatly consistent with this interpretation. In spite of these *ad hoc* suggestions, however, the significant differences between the means appear, at face value, to argue against the hypothesis.

Consideration was not given to the possible complicating effects of temporary work decrement (reactive inhibition) in the original formulation of the hypothesis (4), and perhaps rightly so. If, however, one wished to separate learning from work factors, it would be necessary to introduce a rest period before each test point, to permit "complete" dissipation of work decrement. This was deliberately not done in the present experiment, since the intention was to test the ability of the stimulation-maturation hypothesis to predict net performance levels. The present design would not be suitable for the detection of "learning" differences, except at the end of practice, since the introduction of 5- or 10-minute rests at various times during the practice of groups given shorter intertrial rests would change the intertrial rest conditions.

Summary and Conclusions

The purpose of this experiment was to test the hypothesis of stimulation-induced maturation. Five groups of 21 Ss received 45, 30, 23, 13, and 8 one-minute trials on a compensatory pursuit task, with intertrial rest intervals of 0, ½, 1, 2½, and 5 minutes, respectively. The results show that learning is more nearly proportional to amount of practice than to elapsed time, and do not appear to be consistent with the maturational hypothesis. A rest of 30 seconds between trials led to better performance than did continuous practice, but increases in the duration of interpolated rest from ½ minute up to 5 minutes did not result in further improvement,

whether performance was plotted against time of practice or total elapsed time. Our results appear to disagree with those of Doré and Hilgard despite considerable similarity of task. Since this disagreement does not seem easily explainable in terms of difference of experimental task or conditions, it is suggested that the stimulation-maturation hypothesis be reconsidered.

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ADVANTAGE OF EARLY OVER LATE PERCEPTUAL EXPERIENCE IN IMPROVING FORM DISCRIMINATION[1]

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THIS study was undertaken to discover whether visual experience with specific forms will have different effects depending on the age at which it occurs. Interest in transfer of training has been revived by a renewal of research on the effects of early experience; this was largely stimulated by Hebb's (5) emphasis on the positive effects of abundant early experience. Previous studies by the present writer (3) have further suggested that the amount of transfer depends, not only on the amount of early experience, but also on other factors such as the relationships between the experience and the transfer task.

Hebb has also argued that a complete theory of learning must account for the generalized effects of transfer from infant experiences, as shown in two experiments reported by him. One study found that rats with "richer" early experience had superior intelligence; the other found that late-blinded rats were superior to early-blinded rats in problem-solving ability. From the combined results of these and other studies he infers that the transfer effects of infant experience are more permanent, generalized, and important than those of adult experience (5, p. 229). This conclusion is apparently supported by the inferiority in pattern perception of chimpanzees (7) and rats (4) reared in darkness, and of congenitally blind humans given vision at adulthood (5, pp. 31–35).

Certain questions need answering, however, before this interpretation can be fully accepted. In the first place, Hymovitch (6) did not find differences in problem-solving between late- and early-blinded rats. Secondly, and more important, we cannot confidently conclude that infant experience is more important than adult experience even if we accept the evidence reported by Hebb, for in these studies the adult animals did not have experiences comparable to those of the infant group. That is, the rats blinded in infancy had no adult visual experience comparable with the infant visual experience of the late-blinded group, since the former were blind for the rest of their lives. In order to conclude that infant experience is more effective than adult experience, we must com-

[1]This investigation was supported, in part, by a grant from the committee for the advancement of research at the University of Pennsylvania. The author wishes to thank his colleagues, Drs. Francis W. Irwin and Morris S. Viteles, for suggestions which materially improved the study.

pare the performance of a group which had specific experiences during infancy with that of a group which had like experiences during adulthood. The present study was designed to permit such a comparison, and thus put Hebb's proposition to a more adequate test. Specifically, the following hypothesis was tested: *A group of rats having perceptual experience with specific two-dimensional forms during infancy will be superior in form discrimination to another group of rats having comparable experience during later life.*

General Design

To test the hypothesis four groups were used in a two-factor design. Group EF was given the specific training during early life; Group LF was given the training during later life. Groups EC and LC were the controls for Groups EF and LF respectively, and did not receive the specific training. The experimental groups were tested at different ages, so that the delay between the end of the form experience and testing would be the same for each; hence two control groups were used.

Rearing

Four groups of hooded rats were reared under different conditions from birth until they were tested. There were 11 rats in each group. All animals were reared in metal-walled cages 15 in. long, 9 in. wide and 9 in. high. The top was made of ¾ in. hardware cloth, and the floor of ¼ in. hardware cloth. All animals were thus unable to look at forms outside their cages, since the only openings were at the top and bottom where there were no forms. The rats, born in the psychological laboratory of the University of Pennsylvania, were all offspring of pregnant females received from the Rockland county farm in New York.

Group EF constituted the early form group. These animals, the offspring of one mother, were born in one of the cages described above. However, the cage was modified so that the animals were exposed to specific two-dimensional visual forms from the moment their eyes opened. This was done by making two grooves and inserting a transparent plastic sheet near the two short ends of the cage. The specific forms were placed between the plastic and the metal wall so that the animals could see the forms without touching them. Each plastic sheet, 8¾ in. wide, 9¾ in. high, and ⅛ in. thick, was located 3/16 in. from the end of the wall. The forms, approximately 3 in. high, were painted white on black cardboard 4½ in. wide, 4½ in. high, and 1/16 in. thick. Four different forms were exposed. A triangle and a cross were located side by side at one end of the cage. The other end contained a circle and a square side by side. (Form discrimination between circle and square was not tested since the experiment took too long.) The cards were periodically moved and rotated so that the animals saw each figure at each of the two ends and also in different spatial orientations.

Although the forms were in the cages from birth onwards, the visual experience of the rats did not begin until their eyes opened when they were 16 days old. They received this visual experience for 25 days until they were 41 days old.

When the animals were weaned at 21 days old, each was transferred to a single cage, identical with the one in which it was born, and reared individually for the rest of its life.

It should be pointed out that the litter comprising the early form group was not split into components of the other three groups, since we wanted the early form group to have the visual form experience from the time when their eyes first opened, when they were too young to be weaned. However, the split-litter technique was used in forming the other three groups, so we have a fairly adequate control of genetic backgrounds.

The other three groups were split from the offspring of three pregnant females. Each litter was born in a cage identical to that housing Group EF, except that the specific two-dimensional forms were absent. The three litters were weaned at 21 days and split into the three groups. Each rat was then reared individually until the end of the experiment.

Group LF, the late form group, were reared without the visual forms in their cages until they were 41 days old. From this date, however, the forms were inserted in their cages and remained there for 25 days, until the animals were 66 days old. These animals thus received experience with specific two-dimensional forms similar to that received by Group EF, except that it came later during their life span.

Group EC, the early control group, were reared like Group EF but never exposed to the forms.

Group LC, the late control group, were reared like Group LF but never exposed to the forms.

There were 7 males and 4 females in Group EF, while each of the other three groups contained 6 males and 5 females.

The four groups were treated alike in all other respects. Each cage had good illumination, provided by standard fluorescent lamps located above the racks containing the cages, and by daylight coming through the windows of the laboratory. The lights were turned off at night. The animals were never deprived of water during the rearing period. One water bottle, filled daily, was located on the side of each cage. The diet consisted of purina dog chow pellets and lettuce, placed in the cage. One side of the cage contained a metal door for transferring food and rats. All animals were momentarily handled once a week when the cages were cleaned.

Procedure

The testing procedure for Group EF and Group EC began when the rats were 41 days old, whereas the testing procedure for Group LF and Group LC began when they were 66 days old.

The animals were first placed on a 23-hour food deprivation schedule for 3 days. At the end of each day they were fed purina dog chow and lettuce for one hour. At the end of this 3-day period the form-discrimination training began.

A four-unit visual discrimination apparatus, similar to one described earlier (**2**, p. 333) was used, except that the alleys were closed and that the rat was mildly shocked if it chose the incorrect alley.

A floor plan of one unit of the maze is presented in Figure 1.

The maze was built of ¾ in. pine wood painted a flat black and was elevated 3 feet from the ground. The alleys were closed off from the surroundings by walls 5 in. high. The top of the maze consisted of removable sections made of ¾ in. wire mesh. The swinging doors were 6 in. long and 4 in. high, and contained the black discrimination cards. These were 5¾ in. long, 4 in. high, with white forms 3 in. high, painted on them. A starting box 6 in. wide, 4 in. high and 8 in. long fitted into the first discrimination unit, which was like the one shown in Figure 1. This was followed by three more discrimination units, making four in all. The last unit led directly to the

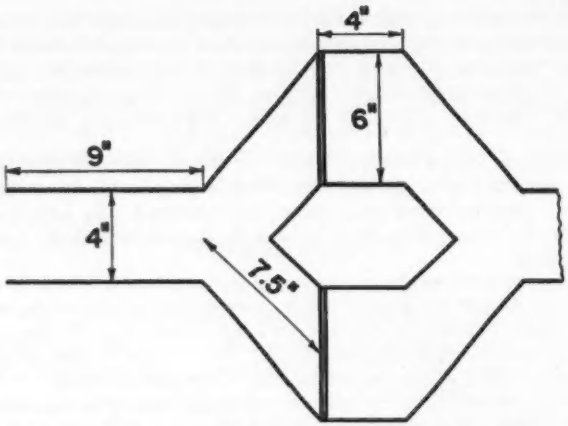

FIGURE 1. Floor plan of one unit of the discrimination apparatus. The solid double lines indicate the positions of the swinging doors which contained the discrimination cards.

goal box which had the same dimensions as the starting box. Each discrimination unit was wired so that the incorrect alley could be electrically charged by throwing a knife switch.

The animals received four days of preliminary training on this maze. They were always trained after having been deprived of food for 23 hours. During the first three days the animals were trained merely to push through the light-metal swinging doors. There were no forms on the doors and no shock was given. The animal was placed in the starting box and could choose any alley since both swinging doors were unlocked. If the animal hesitated at the doorway, the experimenter helped it by partially pushing the door open. Each animal received one daily trial of pushing through the doors of all four units before it was rewarded with purina dog chow in the goal box. At the end of each daily trial it was fed for one hour. On the fourth day the discrimination cards were inserted on the swinging doors. In each discrimination unit one door contained an upright triangle; the other door contained an upright cross. The door containing the triangle, which was positive, was unlocked, whereas the one containing the cross, which was negative, was locked. However, the animal was not shocked for an incorrect choice but was permitted to correct itself and go to the next unit. After the last choice it was rewarded and fed for an hour.

After this preliminary training the following two discrimination tests were carried out using the same apparatus. In both tests each animal was given one trial per day after 23-hour food deprivation, was rewarded with a purina dog chow pellet in the goal box, and then fed for one hour.

Form Discrimination

The animal was required to discriminate between the upright triangle which was positive and the upright cross which was negative. The forms were identical with the ones which had been located in the living cages

of the experimental group. The rat was placed in the starting box and ran to the bifurcation from which it was to make its choice. If it chose the triangle, it could push through the one-way swinging door quite easily. If it chose the cross it was shocked, and also found the door locked. There were four possible errors in one trial. The correction method was used and backtracking was not scored an error. The left-right positions of the positive and negative forms were randomized according to a predetermined sequence. The criterion of learning was two consecutive errorless trials, i.e. eight consecutive correct choices. Time scores were also recorded from Trial 5 onwards.

Form Generalization

After each animal had reached the form discrimination criterion the forms were rotated clockwise through 90°. The number of errors each rat made in 10 trials was recorded.

Results

Because the results of the two tests exhibited similar trends they are presented together. Table I shows the mean number of errors for each group on each test; Table II shows the p values obtained when the major differences between groups were tested for significance by t ratios.

TABLE I

Means and Standard Deviations for the Four Groups on Form Discrimination and Generalization Tests Form

Group	Form disc.		Form gen.	
	Trials	S.D.	Errors	S.D.
EF	33.8	4.2	4.7	2.0
LF	39.4	5.9	11.1	1.7
EC	48.2	3.5	19.7	3.2
LC	46.5	5.9	19.6	3.4

TABLE II

Significance (t test) of Differences between Groups in Form Discrimination and Form Generalization

Groups compared	Form disc.	Form gen.
EF *vs.* EC	<.001	<.001
LF *vs.* LC	<.01	<.001
EF *vs.* LF	<.05	<.001
EC *vs.* LC	>.1	>.1

These tables show that: (*a*) the animals with specific visual experience (experimental groups) were superior in form discrimination and generalization to the animals without this experience (controls); and (*b*) the animals with earlier visual experience (Group EF) were superior to those having the experience later (Group LF). The interaction between age and experience suggested by the latter finding was tested for significance by a double-classification analysis of variance. The F ratio obtained for interaction was 5.21, which is significant ($p < .05$).

It will be noted that the experimental groups exhibited good form generalization whereas the control groups did not. This is in agreement with the author's previous finding (2) that form generalization as well as form discrimination can be improved by early perceptual experience.

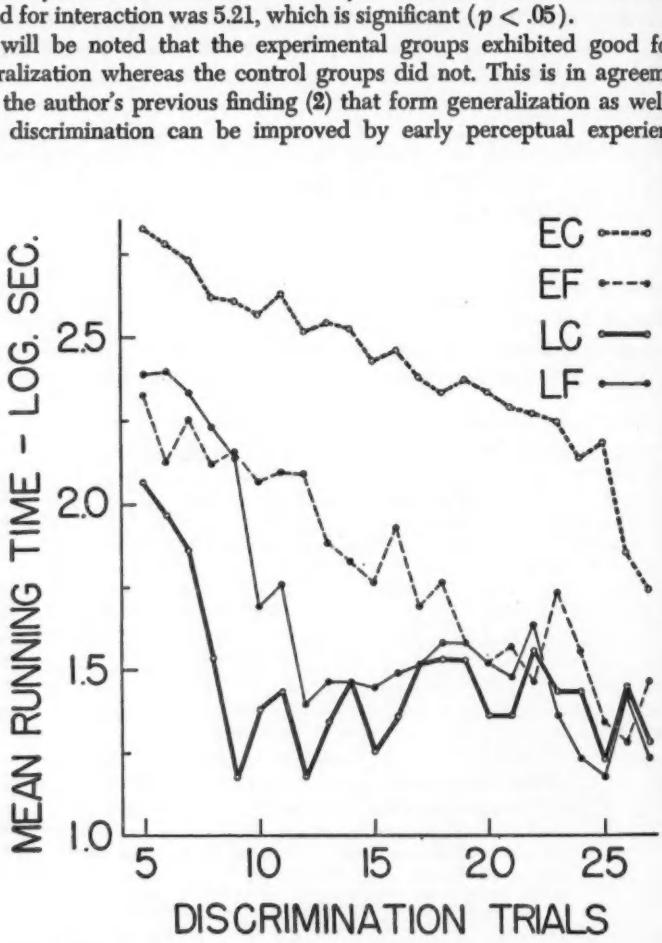

FIGURE 2. Mean running time per trial for each of the four groups between Trial 5 and Trial 27.

These results are interesting, since Field's (1) adult rats showed form generalization only after special and prolonged training.

It is also clear from the scores of Groups EC and LC that age of testing did not significantly affect the performance of the rats on the two tests used in this study.

The age at which the animals were tested did, however, make a difference in their speed of running, as can be seen from the graph in Figure 2. Running times for the first four trials were very long and are not shown; the graph shows running times for the remaining discrimination trials during which every rat was still being tested. It can be seen that there was no difference in the running times of the two older groups (LF and LC), and that the older rats were much faster runners than the younger during the early part of the testing procedure. However, the early experimental group improved rapidly and were running as fast as the older rats by Trial 18, whereas the early control group was still much slower even at Trial 27.

Discussion

We may conclude that rats who have perceptual experience with specific two-dimensional forms are superior in discrimination and form generalization (of those forms) to rats who do not have such experience. Moreover, the specific hypothesis we set out to test has also been confirmed: rats who have this experience during infancy are superior in form discrimination and generalization to rats who have this experience in later life. This is in agreement with Hebb's position.

Confirmation of the hypothesis might conceivably be questioned because of the fact that the early form group was tested at 41 days of age, whereas the late form group was tested at 66 days. It has already been shown, however, that age at testing made no difference in discrimination or generalization; the two control groups performed similarly, except as regards running speed. Whether the early form group would still have been superior if these animals had also been tested at 66 days of age is a question of retention which the present experiment was not designed to answer. A study might well be done along such lines to test the permanence of the effects, but for comparing the effects of early and late experience on form discrimination learning the present design seemed the most adequate.

Why learning is more effective when it occurs earlier in life is problematic; Hebb (5) has offered a neurological explanation. He suggests that infant learning produces changes in the central nervous system, particularly in the cerebral cortex, which permanently influence the course of

later learning, whereas learning occurring later, when cortical organization is relatively complete, has less marked effects.

The present experiment, which varied the time of experience without destroying receptors or the central nervous system, shows that Hebb's psychological theory can be adequately tested on the psychological level, without specific reference to the underlying neurological conditions.

Whatever these may be, it appears that the development of form perception, which is one of the important bases of cognitive development, is a continuous process, at least in the early years of life. That is, the nature of early experience with form determines the effectiveness of later experience in such a manner that later training may be less effective if certain important experiences are meagre or lacking during infancy. Whether early ages are "crucial" in the sense that the specific learning will only occur during that age period, or whether they are crucial because the early learning is a base for what comes later, is an open question to be answered experimentally; such research is being planned.

The above relationship may have some bearing on the concept of psychological "readiness" as used in education, i.e. the notion that training of any kind will be most effective if it is initiated when the organism is maturationally "ready" for it, rather than earlier or later. The present results suggest that the hooded rat might be "ready" for visual form experience soon after its eyes open, and that delay makes such experience less effective as a basis for form discrimination and generalization. Just when the organism is "ready" for other types of experience can best be answered by empirical studies on a variety of species. Many more experiments will be needed to determine the precise nature of the age-experience interaction for other cognitive, and also emotional, experiences.

Finally, we have the fact that the younger animals ran much more slowly than the older animals during the early stages of the discrimination test. Observation suggested that the reason for this longer running time was that the younger animals hesitated longer and more frequently at the discrimination doors, as if they were more afraid of pushing through them, and they appeared, in general, more timid than the older rats. The early *form-experienced* group, however, quickly adjusted to the situation and ultimately ran as fast as the older groups, whereas the early control group did not. If the assumption of greater fear in the younger rats is accepted, one might suggest that the presence of familiar forms on the discrimination doors led to more rapid emotional adaptation in the form-experienced group than in the young control group, with consequent more rapid increase in running speed.

Lest it be argued that the entire results could be explained in "emotional" terms, let us hasten to point out that there appeared to be

little relationship between error scores and time scores; for example, there was a significant difference between the two older groups in errors but not in running times.

Summary

This study tested the hypothesis that rats which had perceptual experience with specific two-dimensional forms during infancy would be superior in form discrimination to rats having comparable experience during later life.

One group of animals was exposed to triangles, crosses, circles and squares from the age of 16 days to the age of 41 days. Another group received the same training between the age of 41 days and 66 days. Two control groups, receiving no visual training, were used. All animals were then tested for visual form discrimination and form generalization of a triangle and cross.

The results indicated that both form-experienced groups were superior to the control groups, and that the early experienced group was superior to the late experienced group. The findings are discussed with respect to Hebb's theory and to the general problem of cognitive and emotional development.

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EMG GRADIENTS AND EEG AMPLITUDE DURING MOTIVATED LISTENING[1]

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EARLIER papers reported electromyographic (EMG) gradients during mirror tracing (1, 2, 9, 11), listening (12, 14), and tracking (13). EMG gradients refer to a progressive rise in muscle potentials from the beginning to the completion of the tasks. Such gradients were observed in arm, neck, chin, and forehead muscles. Bartoshuk summarized evidence which led to his hypothesis that the slope of EMG gradients was indicative of the subject's motivation to do the task (2). Although EMG gradients were usually recorded simultaneously from several muscles, they were not all equally indicative of motivation to do the task. Usefulness of a muscle in this respect varied with the nature of the task.

This experiment investigates the relation between EMG gradients and EEG amplitude in subjects listening to a story. EEG amplitude is used to examine how the subject's arousal state is related to his motivation to do a task. Such a use of the EEG is supported by evidence on the EEG correlates of psychological arousal states (7, 8).

METHOD

Subjects.[3] The Ss were 20 men from the RCAF, 17 to 33 years of age. All had terminated their formal education after several years in secondary school.

Apparatus. Muscular activity was recorded as electromyographic potentials (EMGs) between bipolar surface electrodes. The EMGs were amplified and recorded continuously on an Edin ink-writing electromyograph. Connected in parallel with this primary unit were vacuum tube integrators using the condenser-charge principle. These integrators were set to provide an average measure of the EMGs for each consecutive 10-second period. Standardized placements were used to record EMGs from forehead (*frontalis* muscle), chin, right and left forearm extensors (3).

Bilateral EEG tracings were recorded from frontal and parietal placements. To facilitate quantification, the EEGs were also integrated for each consecutive 10-second period. Filters in the Edin electromyograph and in the integrators were used to restrict the range of frequencies integrated. For an input of constant voltage, the

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[3]The co-operation of the Royal Canadian Air Force personnel who served as subjects and of the officers who assisted with arrangements is sincerely appreciated.

peak response was at 20 cps.; the response diminished progressively to zero as the frequency approached 5 and 45 cps. Consequently, the integrated measure did not differentiate alpha from beta waves in terms of frequency, but it did measure EEG voltage. This provided an objective estimate of average EEG amplitude.

To facilitate comparisons between studies we made a tape-recording of the detective story used by Wallerstein (14). It lasted ten minutes and was presented three times to each S.

Procedure. On arrival S was told that we wanted to study his muscle tension and brain waves while he rested and while he listened to a story. Electrodes were attached and S lay on a comfortable bed. More detailed instructions followed; these encouraged S to relax and not to think about anything during the silent periods, but to try to remember as much of the story as possible, since we would question him about it later. This was to enhance S's motivation to listen.

The sequence of events was as follows: (*a*) three minutes of resting in silence; (*b*) a brief recorded summary of earlier instructions to lie quietly, with eyes closed, and to concentrate on the story; (*c*) another minute of silence; (*d*) the ten-minute story; (*e*) two minutes of silence. This was outlined to S so that he would know what to expect, and could relax without anxiety. When the initial cycle was completed, S was informed that the entire cycle would be repeated. Before the repetition there was a five-minute interval in which S was allowed to open his eyes, move about, and talk. After completion of the second cycle, S was requested to listen to the story once more. After completion of the third cycle all Ss were questioned about the story's content and asked to rate their interest in it during each of the three presentations.

Treatment of data. Gradient slope was measured as the EMG change during the period from 30 seconds after the story's start to its end, 9½ minutes later. This procedure allowed for any transient EMG changes associated with the start of listening. Throughout the analysis nonparametric statistics were used; all probability figures are based on two-tailed tests.

Results

Forehead. In the first presentation of the story, gradient slope during listening was positive in 11 Ss and negative in 9 Ss. The largest positive gradient rose 21.9 microvolts and the largest negative gradient decreased 7.8 μv, a range of 29.7 μv between extreme cases.

Although forehead recordings must include some EEG potentials, two facts support the conclusion that positive gradients originated in EMG changes. The first concerns frequency: forehead and forearm EMGs had a similar modal frequency of 45 cps.; positive forehead gradients were due to increased amplitude of these 45 per second spikes (Figure 1). Equally important is the fact that although EEG tracings could include frequencies up to 45 cps., there were no gradients in the frontal or parietal EEGs to correspond with the positive gradients recorded from the forehead (Figure 2).

Samples of parietal EEGs in Figure 3 illustrate the fact that Ss with positive EMG gradients had low voltage EEGs, whereas Ss with negative EMG gradients had high voltage EEGs. Integrated measures revealed

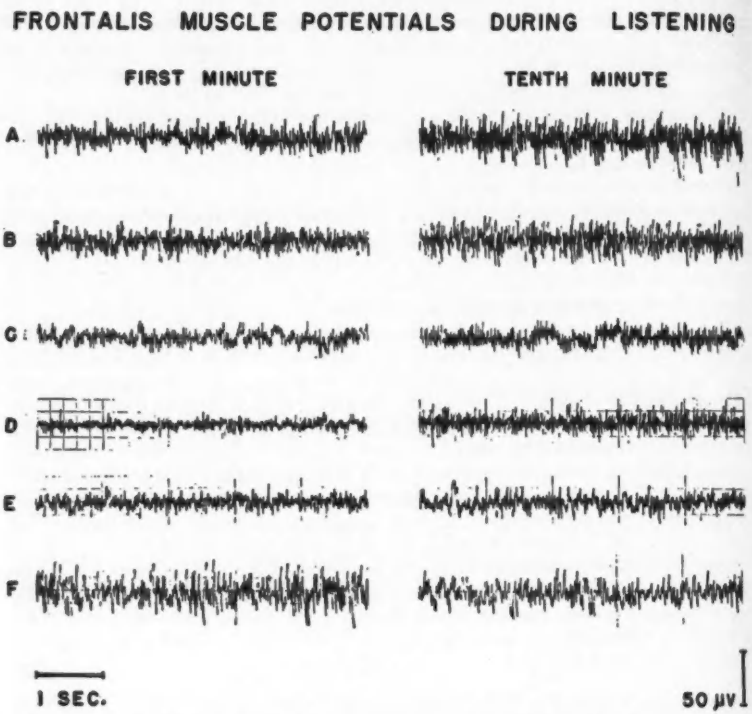

FIGURE 1. Paired samples of forehead tracings during first and tenth minute of listening. Average frequency of spikes (about 45 per second) is the same as that seen in ink-tracings from surface electrodes over limb muscles.

that this inverse relation between slope of EMG gradients and EEG voltage was significant for parietal (P .001) and frontal (P .005) EEGs. Since the average EEG voltage of 20-second samples remained relatively constant before, during, and after listening, the pre-listening EEG voltage predicted which Ss later had positive EMG gradients during listening.

Slope of EMG gradients and EEG voltage were inversely related in the first presentation of the story, but not subsequently; on the two repetitions EEG voltage remained unchanged whereas EMG gradients varied. Inter-hearing correlations were accordingly much lower for EMG gradients than for EEG voltage. These correlations were, for EMG gradient slope: .24 between first and second hearing, .23 between first and third, and .53 (P .03) between second and third. Corresponding correlations of parietal EEG voltage were: .94 between first and second hearing, and .95 between

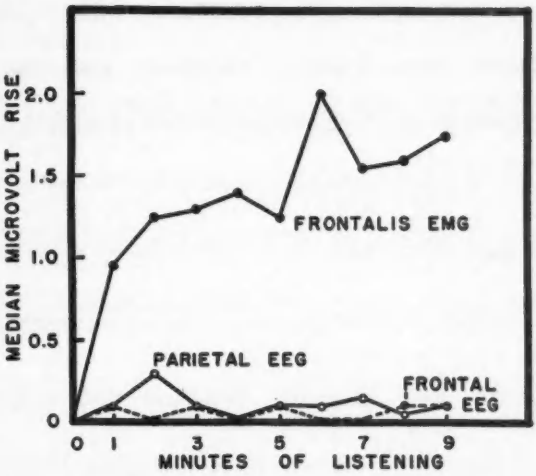

FIGURE 2. Changes in EEG voltage after the first minute of listening in ten subjects with positive forehead EMG gradients.

second and third. Similarly, frontal EEGs gave a correlation of .86 between first and second hearing, and .90 between second and third. These results show that inter-hearing changes in the slope of EMG gradients were independent of EEG voltage.

Changes in forehead EMGs were not restricted to the listening period. In all three hearings, forehead EMGs increased slightly (0.6 μv) but significantly (P .02) from the resting period to the "expectancy" period, which followed instructions that the story would start in one minute. Moreover, in the first two hearings, there was a further increase of 0.8μv (P .01) immediately at the start of the story. Average EMG level was the same in the resting period preceding each "expectancy" and listening period.

Corresponding to these changes in forehead EMGs were three significant changes in EEG voltage, all in the first hearing. Parietal EEG voltage decreased slightly (P .01) from the resting to the "expectancy" period, whereas frontal EEG voltage remained unchanged. Then both parietal (P .05) and frontal (P .01) EEG voltage increased slightly at the start of the story.[4]

[4]A qualitative inspection of the primary EEG tracings showed that this rise was not consistently due to any one of: increased alpha amplitude, increased amount of alpha, or increased frequency of the brain waves. Such an increase in EEG voltage at the start of a task has also been observed by Ford (4) and by Stennett (unpub-

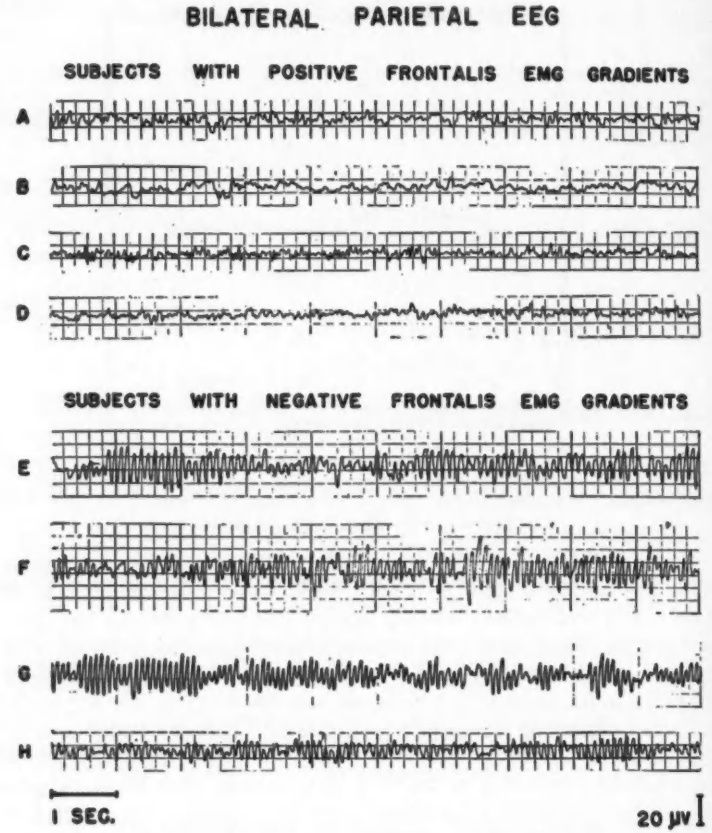

FIGURE 3. Samples of parietal tracings from eight subjects illustrating the relation of EEG voltage to positive and negative forehead EMG gradients.

Other EMGs. Recordings from chin revealed positive EMG gradients in approximately half the Ss in each hearing. The range of gradient slope was about 40 μv. In spite of these superficial similarities between forehead and chin gradients, the slope of chin gradients was independent of EEG voltage in all hearings. This finding is consistent with the low correlations

lished data collected at the Allan Memorial Institute). These findings oppose the prediction of flattening of the EEG that one would normally make from an arousal theory; although the increases are apparently small, their regular occurrence calls for further investigation.

between slope of forehead and chin gradients: these were .35, .32, and .27, for the three hearings.

Both forearm extensors showed an average decrease of 0.9 μv (P .05) during the initial hearing. In later hearings there were only small random fluctuations. These forearm EMG changes were not related to EEG voltage.

Retrospective data. Seventeen Ss reported which of the three presentations of the story they found most and least interesting. Eleven of these Ss had steeper forehead EMG gradients in the most interesting hearing, one S showed no difference, and five Ss had steeper forehead gradients in the least interesting hearing. The tendency towards steeper forehead EMG gradients in the most interesting hearing was not significant, but it was in the same direction as Wallerstein's significant results for 30 Ss (2, 14). Chin gradients did not correlate with interest; this also agrees with Wallerstein's data. In relation to the EEG data, it is important to note that 16 Ss reported feeling alert; of these, nine had low and seven had high voltage EEGs. Lastly, recall scores obtained after the third hearing did not correlate with any of the physiological measures.

Discussion

Although positive forehead EMG gradients occurred in all three presentations of the story, their slope and EEG amplitude were inversely related only when subjects heard the story for the first time. In suggesting an explanation for this finding, we will make use of three other empirical conclusions about forehead EMG gradients which are based on the combined data from Wallerstein's (14) experiment and our own.

1. Forehead EMG gradients were significantly associated with listening to either a story or essay: positive gradients occurred in 38 out of 53 subjects; the mean rise for the 53 subjects was 1.6 μv (P .0002).

2. The slope of forehead EMG gradients was at least partly determined by interest in the material listened to: 33 out of 47 subjects[5] had steeper gradients (P .02) in the hearing they found most interesting than in the one they found least interesting. This supports the hypothesis that forehead EMG gradients are indicative of motivation to listen.[6] This, of course, does not mean that positive forehead EMG gradients during

[5]Only 47 of the 53 Ss were available because 6 Ss had been inconsistent in their ratings of interest.

[6]A corollary of the above conclusion is that with enough repetitions of the same story both interest and forehead EMG gradients should disappear. It was not possible to support this by demonstrating a significant decrease in gradient slope from the first to the third hearing. This was because reported interest did not decrease for all subjects from first to third hearing.

listening were paralleled by growing interest in the story; to test this it would be necessary to divide the story into several sections each of which could be rated for interest.

3. Forehead EMGs were more closely associated with listening than either forearm or chin EMGs. Forearm extensors in both studies consistently failed to give positive EMG gradients during listening. Positive chin gradients tended to occur in the first hearing (P .10), and occurred reliably in later hearings (P .01). However, the slope of chin gradients was not related to interest ($N = 41$), forehead EMG gradients ($N = 19$), or EEG voltage ($N = 19$). In view of these negative relationships with other variables, no interpretation of the chin gradients is offered.

The interpretation made of the EEG data rests on the subjective reports of our subjects, which would locate them on Lindsley's psychological continuum between relaxed wakefulness and alert attentiveness (8). In this range Lindsley shows EEG amplitude as decreasing from relaxed wakefulness to alert attentiveness.[7] Acceptance of this inverse relation between EEG amplitude and arousal level as a working hypothesis suggests the following interpretation of our data.

In the first hearing, arousal level and occurrence of forehead EMG gradients were positively related. Subsequently, although arousal level did not change, EMG gradients were no longer related to arousal; apparently other factors having to do with change of interest in this particular story became more important. These conclusions are in general agreement with Wallerstein's distinction between reported wakefulness (which was related to the resting level of forehead EMGs) and sustained attention to the story (which was reflected in the forehead EMG gradients). He also concluded that sustained attention to the story was not very closely related to wakefulness, but did require at least a certain degree of wakefulness.

Hebb's recent paper on motivation provides a basis which can be used for a fuller explanation of these results (6). He postulates that every sensory event has a cue function, which guides behaviour, and an arousal function, which activates the arousal system. The latter is said to act as a necessary energizer without specific guiding properties of its own; it must provide an appropriate degree of arousal to facilitate the cue function, otherwise the latter cannot exist. Arousal in this sense is assumed by Hebb to be synonymous with general drive state. Present results suggest that repetition of a story changes primarily the cue function which

[7]Some possible limitations in the use of EEG amplitude as an indicant of arousal state are suggested by the fact that EEG amplitude can be related to other factors, such as type of imagery used—subjects with predominantly visual imagery would also give low amplitude EEGs (10).

guides thought, so that even without any change in arousal state, the story becomes less able to direct sustained thought—possibly because of short-circuiting in the phase sequence associated with the story (5, pp. 228–229).

Accordingly, our motivational hypothesis of EMG gradients should be reformulated as follows. The concept "motivation to do the task" has the dual properties of a vector, that is direction and magnitude. When we use the slope of EMG gradients from a specified muscle as an indicator of motivation to do a particular task, then a change in the slope of the EMG gradients could be due to a change in the subject's motivation which involves direction only (i.e. Hebb's cue function), magnitude only (i.e., arousal state), or both. Independent objective measures of arousal state and cue function would be required for a rigorous analysis of how they jointly determine motivation to do the task, as indicated by the slope of EMG gradients. Further analysis of these relations might be more profitably done with motor tasks, which would provide a measure of performance, and with experimental manipulation of the subject's arousal state.

Summary

EMG gradients, defined as progressive increases in muscle potentials during a given task, have been observed in mirror tracing, tracking, and listening situations. Bartoshuk's hypothesis, that the slope of some EMG gradients is a function of motivation to do the task, has been supported by data on interrupted tasks, performance scores, interest in the task, and the effect of incentives. The present experiment examined the relation of EMG gradients to arousal (as indicated by EEG amplitude).

Combined results from a previous experiment by Wallerstein and this one demonstrated the significant occurrence of forehead EMG gradients during listening. A positive correlation between gradient slope and reported interest supported the hypothesis that forehead EMG gradients were indicative of motivation to listen. In the present experiment the slope of forehead EMG gradients was inversely related to EEG amplitude in the first hearing of the story, but not in subsequent repetitions of it. It was concluded that slope of forehead EMG gradients (and therefore motivation to listen) depended on both arousal state and the cue function postulated by Hebb.

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THE DISTINCTION BETWEEN "CLASSICAL" AND "INSTRUMENTAL"

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THIS note draws attention to an inconsistency in classifying conditioned responses which relates to an important question concerning the mechanism of learning.

Hilgard and Marquis (2) crystallized an idea towards which psychology had been feeling its way for several years, in the distinction of *classical* from *instrumental* conditioning. This separation has had an increasing emphasis in the literature, but discussion of the point has perpetuated a contradiction. Classical conditioning is identified in two ways: (A) it is the procedure of Pavlov, and (B) it is the procedure in which the CR is irrelevant to occurrence of the US or reinforcement (2, p. 27). Instrumental conditioning, on the other hand, is obtained by a procedure in which the CR determines whether the US, or reinforcement, will occur or not.

The contradiction is produced by the fact that salivary secretion as studied by Pavlov is, by definition B, *not* classical, but instrumental. Not only must the dog reach for and seize the food, in Pavlov's procedure; he must also secrete saliva if the food is to be positively reinforcing. Dry food in the mouth is noxious rather than rewarding and cannot be swallowed. Both the primary reinforcement of food in the stomach and, to a great extent, the secondary reinforcement of taste are therefore contingent upon the specific response (salivation) that is being conditioned.

Schlosberg (4) made this point (though he appears to have underestimated the complete necessity of salivation if the food is to be a reward instead of a deterrent). He also pointed out that the same considerations apply when sand or acid in the mouth is used as UCS. The secretion of saliva permits ejection of sand, or dilutes the acid to a point at which it is innocuous.

In the case of acid, we are clearly dealing with the escape-avoidance paradigm that comes under the instrumental heading. At first, the acid would enter the mouth when the ordinary amount of saliva is present and would burn. The secretion of saliva would then dilute it, constituting "escape" from the stimulus. In later trials, copious secretions as CR would mean that the acid would be diluted immediately on entry to the mouth and no noxious stimulation at all would occur: "avoidance."

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To think of this procedure as not instrumental commits the error of thinking of "acid in the mouth" as the stimulus. The acid is put in the mouth whether there is salivary secretion or not, and this sounds like the classical procedure (i.e., by definition B). But the acid is only a stimulus when it comes in contact with receptors at a sufficient concentration to excite them. Similarly, pupillary contraction to light in the eye, or an eyeblink to a puff of air on the cornea, is instrumental; for the pupillary contraction is "rewarding" when it cuts down a too-high intensity of light, or the blink when it prevents air from reaching the cornea. The stimulus here is not the light or the air directed at the eye, but the amount of energy acting on the receptors. This energy is partly or wholly controlled by the response that is being conditioned.

It thus appears that classical conditioning—classical by definition B—is not what was worked with in the classical period and is not Pavlovian. It is to be found only in those later experiments in which the electrode that gives electric shock is fastened to the animal, the shock being delivered whether the CR is made or not, or those experiments in which the animal is unable to escape, even momentarily, from an electrified grid. The early American experiments in which electric shock could be escaped were, contrary to what is said in the literature, not a change in principle from Pavlov's acid-in-the-mouth method.

If therefore the theoretical importance of the difference between Pavlov's results and those, say, of Miller (3) or Brush and Solomon (1) is what it is thought to be, the difference cannot be accounted for on the basis that in one the animal's response is unrelated to reward, in the other that it determines the occurrence of reward. It may instead be due to differences between autonomic and skeletal responses. More probably, in view of such an analysis as Schlosberg's (4), it may be the voluntary-involuntary dichotomy that is involved, and for purposes of discussion the "voluntary" response could be defined as the one that is subject to a prompt on-off control by learned processes.

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HUMAN MOTIVATION AND FREUD'S THEORY OF ENERGY DISCHARGE[1]

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PSYCHOANALYSTS and clinical psychologists are generally critical of experimental psychology, not only with respect to its methods of approach, but also with respect to the basic constructs underlying behaviourist theories of motivation and conduct. Some of these criticisms may well be justified, but the clinicians do not always realize that the basic concepts underlying Freudian psychology reflect a theoretical approach closely similar to that found in Pavlov's reflex theory and in behaviourism. Both behaviourist psychology and psychoanalysis worked out theories of behaviour in terms of concepts which did not originate in the study of behaviour itself, but were borrowed from sciences such as physics, which study phenomena with quite different characteristics. It is mainly due to this fact that current behaviour theories conceive of motivation as need reduction in the form of energy discharge—an essentially physical concept.

In a previous publication (**14**) I have tried to show experimentally that the concept of need persistence, in addition to that of need reduction, is essential in human psychology. This is because the cognitive elaboration of needs in man creates a special form of task or project in which these needs exist and operate. In the present paper I wish to do something similar with regard to clinical psychology, and particularly psychoanalysis; namely, to examine the insufficiency of the Freudian notion of energy discharge for explaining human motivation. The concept of energy discharge, which dominates psychoanalytic theory and even the experimental psychology of motivation, originates in Freud's theory of the so-called mental apparatus, and this is based on his earlier physical approach to physiology and neurology.

Ernest Jones, in his brilliant biography of his master, agrees that Freud, in constructing his psychological theory, did not renounce the physical principles borrowed from the Helmholtz School of medicine; but Jones believes that Freud's "emancipation from this influence consisted . . . in becoming able to apply these physical principles empirically to mental

[1]One of a series of lectures on "Motivation and cognitive factors in human behaviour," given at the University of London, December, 1955. Portions of this paper were also presented at the 1955 congress of the British Association for the Advancement of Science in Bristol.

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phenomena" (12, p. 45). This opinion is over-optimistic. In science one does not "empirically apply" principles to facts; one uses the principles to build up a theoretical framework in which the facts are perceived and interpreted. And this was the case with Freud. His theoretical thinking about clinical findings was channeled along paths already marked out in part by his earliest physical approach to the human organism. In order to show this, we must examine Freud's theory of the mental apparatus, and particularly his concept of the relationship between the behaving personality and the outside world.

Theory of the Mental Apparatus

When Freud tried in 1924 to define the contribution of psychoanalysis to the understanding of human behaviour, he asserted that this contribution could be only a partial one, and that psychoanalysis did not pretend to be a complete theory of behaviour. Psychoanalysis is concerned, he said, with only one aspect of behaviour—the unconscious mechanisms of the libido. Gradually, however, psychoanalysis broadened its field of investigation, and in recent years, as is well known, it has shifted its interest and emphasis from the impersonal impulses of the libido to the more personalized processes of the ego.

Whenever a science enters such a new phase of development, the basic concepts underlying its previous stage stand in need of revision. The new developments in modern physics, for instance, required that the concepts of time and mass be revised. And, as psychoanalysis becomes a psychology of the ego, several basic concepts of its theory of behaviour also demand revision. This is particularly true of Freud's concept of the so-called "mental apparatus," a concept borrowed from the Helmholtz School of physics and physiology, of which Freud, through his master Brücke, was a pupil and collaborator before he became a psychologist.

Clinicians, who are seldom concerned with Freud's basic theoretical concepts, may regard criticism of them as an attack on psychoanalysis as a clinical psychology. This is not so; I fully appreciate the extraordinary merits of the Freudian approach. If I say nothing about it here, it is because my only purpose is to examine certain concepts which Freud himself called "provisional" and "of the crudest nature." We must not forget that Freud himself was primarily interested in *theoretical* psychology. At 70, as Jones reports, he said that his life had been aimed at one goal only: to infer how the mental apparatus is constructed (12, p. 45). To concentrate, therefore, on the theoretical basis of his theory is anything but alien to the spirit of psychoanalysis itself. Moreover, there

seems to exist a conflict in Freud's theoretical thinking between his physical approach to the mental apparatus and his discovery of the psychological, meaningful character of mental or behavioural processes. This conflict was never resolved, since Freud conceived of the "scientific" character of his system as dependent upon the physical nature of his basic concepts. Hence his general theorizing is a compromise between his masterly new insights and the scientific concepts of his time; he had to pour his new wine into old and borrowed vessels. A thorough revision of his basic concepts may, therefore, bring them into closer agreement with the fundamental orientation of his system.

The insufficiency of Freud's behavioural theory is rooted in the fact that he originally conceived of the "mental apparatus" as a closed physical system, and not as a behaving subject fundamentally exposed or "open" to the outside world. He looked at man primarily as an "apparatus" whose chief concern is to get rid of the outside world. This strange view is understandable when one realizes that Freud did not originally view man from a behavioural standpoint. As a pupil of the Helmholtz School of medicine, he first looked at the organism, and especially the nervous system, from the standpoint of Helmholtzian physics and physiology, in which conservation of energy and the equilibrium of forces were the basic principles of organic functioning. This view Freud later applied to the mental apparatus, conceiving it as a closed physical system of forces in a state of equilibrium. The fundamental law of this system is that it avoids all that disturbs its dynamic equilibrium, that is, all stimuli. As soon as its "quantity" of energy is increased by stimulation, the system tries to get rid of it by discharging the energy in motor reaction.

Therefore, Freud argues, psychological functioning or behaviour is to be conceived of on the model of the reflex arc; the mental apparatus is primarily a reflex apparatus, characterized by a kind of activity in which sensory stimulation is immediately converted into motor discharge. Since its immediate reaction to stimulation consists in trying to get rid of it, the ideal status of the apparatus is to be free of stimulation. Thus the primary attitude of mental or behavioural life towards the stimulating outside world is characterized by a reaction of flight from stimulation (*Reizflucht*).

These basic Freudian ideas were first expressed in neurological terms (5). The neurons, Freud said, tend to be "empty" of excitation. As soon as they are "cathected," i.e., "filled" with a flow of excitation by stimulation, the apparatus starts its activity in order to discharge the flow. Thus cathexis is primarily the fact that some neurons which tend to be empty

are filled by a flow of increased stimulation. Behaviour is primarily a discharge, since the apparatus cannot stand increase in the "quantity" of excitation. Discharge means pleasure; increase of excitation means pain.[2]

One should not exaggerate the importance of the neurological formulations elaborated by Freud in his "Project" of 1895. Indeed, this terminology was disavowed by the author himself, and it seems to be a transposition of psychological facts into neurological terms. More important is the fact that Freud transposed this physical theory of the organism into psychological concepts, and that these concepts form the basis of several essential points in his later theory of motivation and behaviour.

Very early in the development of his theory, Freud came to the conviction that the outside world is not very important as a source of excitation for the mental apparatus. The "quantity" (excitation) in the apparatus is not significantly increased, he says, by contact with the outside world. Stimulation from within the body itself—e.g., organic needs such as hunger and sexual drive—is considered the only important sources of cathexis. In order to get rid of this inner stimulation the organism has to adopt another strategy. Flight from the outside world and direct discharge in motor reaction are no longer effective; on the contrary, the organism can satisfy these needs and terminate inner stimulation only by reaching certain objects in the outside world. Thus the outside world gains a new meaning: it is not only to be avoided from fear of stimulation, but it is also to be sought for as an object of need gratification. This is an idea of great potential importance, which could have given the world a *positive* meaning. That, however, is not the case; Freud's basic conception—avoidance of the outside world—appears again in this context. The primary means of need satisfaction, it turns out, is by internally imagining or "hallucinating" the satisfying object, not through behavioural dealing with the outside world in order to find the object itself (secondary process: reality principle). Freud tries to make this theory plausible by his interpretation of the early mother-child relationship. To us it seems obvious to conceive of this relationship as bringing the infant into psychological or behavioural contact with the world. Not only biologically, but what is more important psychologically, the mother is the channel through which the child enters into the world. One is surprised, therefore, to find Freud conceiving of maternal care and the behavioural child-mother relationship in terms of a closed system, situated in the inner life of the

[2]The theory of the death instinct is to be regarded as a development of Freud's conception of the fundamental law of the mental apparatus, its tendency towards Nirvana and rest. In Freud's original view, the Nirvana principle was identified with the pleasure principle; this was corrected in 1924, when he admitted that tension and energy increase can be pleasurable (8, 372–3).

child. Indeed, he compares this mother-child unit with the embryo in a bird's egg, enclosed and isolated from the world by a shell (7, p. 232, n.).

As regards this primary mode of need satisfaction, by hallucinating the satisfying object, Freud's views are in flat contradiction to behaviour and learning theories. As Thorndike and Hull have shown, satisfying objects influence the organism's behaviour, not by eliciting images, but by making the organism more ready to execute the rewarding behavioural reactions themselves. In other words, the primary process operates on the level of behavioural contact with outside reality, not on the level of imagination or ideation. The latter are much more elaborate and advanced forms of "internalized" reaction.

Theoretical Significance of the Primary Process

My emphasis thus far on the primary process of the mental apparatus may strike the reader as pointless, since Freud himself regarded this primary process (pleasure principle) as only a remnant of earlier stages of development. No mental apparatus, he agreed, actually functions or could function in this primary way. Indeed, man reached his present perfection of behavioural functioning by partially replacing the primary process of direct energy discharge by a secondary one, which underlies inhibition, reality testing, adjusted behaviour, and the creation of the ego. This process, according to Freud, developed out of the primary one because the latter was no longer effective in satisfying inner needs. I need not here describe the well-known secondary process (reality principle); the question is whether Freud's conception of the primary process has any relevance to the understanding of man's actual behavioural functioning.

In answering this question, we should not forget that Freud's genetic view of man's psychological life is a peculiar one. According to his theory, the primary process and the earliest stages of development are never really transcended. The ego, with all the behavioural perfections of the later stages, is nothing more than a tool created by, and derived from, the primary stage itself in order to realize more efficiently the primary goal of energy discharge. This is stated explicitly and repeatedly in Freud's later writings. Owing to the fact that the primary processes are present in the mental apparatus from the first, while the secondary processes only later unfold and come to inhibit and overlay them, *the core of our being*, he says, remains inaccessible to the understanding and inhibition of the preconscious (i.e., to the influence of the secondary process). The part played by the latter, Freud continues, is restricted once and for all (because of its belated appearance) to directing along the most expedient paths the wishful impulses that arise from the un-

conscious (i.e., the pleasurable discharge according to the primary process) (6, p. 103; 7).

One cannot sufficiently emphasize the importance of this basic idea in the Freudian theory of human motivation and behaviour. Freud's view of man is not a genetic one in the ordinary sense of the word; it is a theory in which the "core of our being" remains once and for all on the level of the earliest stage of psychological functioning. To accord with these ideas, ego functioning, thinking, and behavioural contact with the outside world are reduced to the role of secondary formations. This means not only that they are chronologically posterior to earlier stages, which is certainly true, but that once and for all, as Freud says, man as an ego, dealing with the real world and realizing himself in the world, is conceived of as a "detour process," for ever at the service of an organism whose restless activity is nothing but an effort to stop stimulation, to get rid of the world, and to come to rest in a self-enclosed state like the embryo in the egg-shell.

As Fenichel puts it: "The first acceptance of reality is only an intermediary step on the road to get rid of it" (3, p. 35). The outside world is sought only as a means to its own banishment. This is probably what is meant, Fenichel adds, when it is sometimes stated that hate is older than love.

It might seem, at first glance, that Freud had also elaborated a more positive concept of the outside world, in which the capacity to face and accept reality is viewed as the characteristic of a healthy personality. "We have long observed," he says, "that every neurosis has the result, and therefore probably the purpose, of forcing the patient out of real life, of alienating him from reality. . . . The neurotic turns away from reality because he finds it unbearable" (7, p. 230). However, what is primarily meant here by "reality" is not the outside world, but the *inner* world of organic needs, wishes, and impulses, which the patient cannot accept. In fact, Freud states that it was by introducing the concept of repression that he was able to gain insight into the relationship between neurosis and escape from reality (7). The unbearable reality is, therefore, primarily the repressed content of one's inner impulses. Indirectly, however, repression of the inner impulses also entails greater avoidance of outside reality. Repression, Freud argues, can create mechanisms in the mental apparatus which cut off the stream of stimulation from the outside (7). When dynamic contents of the inner world are banned from consciousness, parts of the outside world which are related to these repressed contents are also cut off. In this way the range of interests and experiences is narrowed and perception is limited. This process ends in what Anna Freud has called the "restriction of the ego" (4, ch. 8).

To sum up, then, Freud's view of man's behaviour and motivation is primarily concerned with the equilibrium of forces in a closed physical system, one whose ideal status is undisturbed rest, and whose chief behavioural concern is to get rid of every kind of stimulation or disturbance. Motivation, therefore, is discharge of accumulated stimulation or energy. The outside world is conceived of, first, as a source of stimulation to be avoided, and secondly as a need-satisfying object by means of which the organism, through ego functioning, can get rid of inner stimulation indirectly. This fundamental Freudian view of the so-called mental apparatus, based on Helmholtzian physics, is one of the first concepts to be revised in order to develop, in unambiguous ways, the contemporary trend towards ego psychology.

When Freud in his later writings conceives of neurosis as originating in a conflict between the libido and the ego, this *could* be interpreted in terms of an ego impelled by its own dynamics in conflict with the libido. However, in the framework of Freud's basic ideas, this conflict cannot mean anything other than a struggle between two alternative mechanisms of energy discharge: the id striving for direct discharge, and the ego seeking the same discharge by more expedient and less painful means.

Inadequacy of the "Energy Discharge" Concept

I shall now try to give some positive evidence of the insufficiency of Freud's theory that the primary process and energy discharge represent the core of human motivation and behaviour. Admittedly, this theory does seem to apply in part to the infant in the first days of life. The infant's life alternates between hunger stimuli and sleep. Hunger is inner stimulation which the organism tends to get rid of; getting rid of this stimulation by ingesting food entails pleasure. After satiation the organism is relatively free of excitation, and sleep sets in. Thus, sleep may be considered the ideal state to which the organism tends, and by which it banishes the world. Its behaviour is an effort to rid itself of stimulation in order to be able to sleep again.

Nobody will deny that this kind of psychological functioning may be characteristic of some stages and levels of development. However, there is no reason to conceive of it as eventually dominating and motivating all further behaviour. Moreover, Freud did not realize, as Hartmann says, that "the ability to renounce immediate pleasure-gain in order to secure a greater one in the future cannot be derived from the pleasure-principle alone; not even memories of pain experiences suffice to explain it" (9, p. 381). Development of a secondary process implies the existence, from the very beginning, of potentialities different from the primary process of energy discharge; it also implies cognitive awareness of the

outside world. This is probably the basic source of Freud's theoretical shortcomings: he conceives of cognitive contact with the world, and even of consciousness, in terms of cathecting and discharge of energy.

Contemporary research is more and more emphasizing man's cognitive communication with and exposure to the world as fundamental to psychological life. The need for cognitive and behavioural dealing with objects seems to be a basic one, not merely a detour process. Recent experiments at McGill (**2**, **10**, **11**) show convincingly that man cannot stand the absence of contact with the outside world, even when his organic needs are completely satisfied. Subjects insulated from outside stimulation for 10 or 12 hours were no longer able to behave normally or to think; the experience was so upsetting that some refused to continue. On the other hand, one of the main drives of man, and even of anthropoid apes, as Harlow has shown (**8**), is the need to explore the outside world *for the sake of exploration itself*. These findings seem incompatible with the view that man's primary aim is to get rid of stimulation and of the world.

The Freudian conception of motivation as energy discharge may be adequate to explain some elementary forms of motivation and biological need reduction, but it cannot account for the more constructive aspects of behaviour—the fact that man tries to make something worthwhile of his life, and takes innumerable risks in order to realize his projects. The healthy personality enjoys tensions, not only their discharge; he likes the responsibilities and risks involved in actualizing his potentialities. Indeed, the main fact to be accounted for in human motivation is precisely this constructive impulse, this planning of new realizations, this repeated destroying of the state of equilibrium as soon as it has been reached (**13**). Nor should one forget, as Scheerer argues, "that already the early stages in child development contain gratifications other than tension discharge. In the interaction between perception, mobility, cognition and the object-characteristics as dealt with by the child, the fact that there is more involved than tension release has been shown by the work of C. H. Buehler, W. Stern, Gesell, and others" (**15**, p. 272). Asch points out that the child is most characteristically active when he is free from hunger, fatigue or pain, that the surroundings continually draw him into action, so that he forgets the bruises he suffers, and fails to heed the call to eat. "Children are concerned not simply with doing, but with making and constructing. . . . Their orientation is eminently task-centred. The task itself is the challenge and creates its own suspense and zest" (**1**, pp. 298–9).

I am often struck by the fact that men beyond the age of forty may become rather anxious about what they describe as a certain lack of tension and excitability in their psychological life. As they grow older, a

certain state of "rest" enters the organism. Instead of being considered an ideal state, this phenomenon is felt to be a diminution of the personality. It is the first touch of death and creates its own unrest. Repeatedly these people try, by various kinds of stimulation, more or less artificial, to test their degree of responsiveness and potentiality for excitation. The menace of drowning in a psychological or organic sleep is more than they can stand.

The Ego and the Personality

Finally, let us examine the concept of the ego as developed by Freud in later life, namely as a part of the mental apparatus.

Freud, as is well known, conceives of personality as composed of three so-called agencies or systems. To better understand this conception of personality, we should go back to his earlier writings. In his unpublished "Project" of 1895, Freud finds it necessary to introduce two kinds of neurones in order to account for two different mental functions, perception and memory. Later, in his *Interpretation of Dreams*, the two kinds of neurones became two psychological agencies or systems. "As has already been pointed out elsewhere," Freud says, "there are obvious difficulties involved in supposing that one and the same system can accurately retain modifications of its elements (as is the case in memory) and yet remain perpetually open to the reception of fresh occasions for modification (as is necessary for perception). In accordance, therefore, with the principle which governs our experiment (*Versuch*), we shall distribute these two functions on two different systems. We shall suppose that a system in the very front of the mental apparatus receives the perceptual stimuli but retains no trace of them and thus has no memory, while behind it there lies a second system which transforms the momentary excitations of the first system into permanent traces" (**6**, p. 538).

This way of thinking becomes clear if we recognize that, when Freud speaks of the "obvious difficulties" involved in assigning two functions to the same "system," he is referring to what is said about this problem in the *Studies on Hysteria* (1895). In a reflecting telescope, it is said there, the part that functions as a mirror cannot at the same time be a photographic plate. Similarly, different parts or systems of the mental apparatus must be necessary for the different aspects of psychological and behavioural functioning. Here, once more, we see the primitive mechanical model underlying Freud's psychological theory. What is interesting to us now is that, more than twenty years later, Freud introduces his distinction between the ego and the id with the same terms of "agencies" and "systems" as in the passage just discussed. Thus, in the structure of

the human personality, as in a telescope, different systems must be assumed if we are to account for different aspects of its functioning.[3]

The only point I wish to emphasize here is that this way of thinking, based on physics and physical models, obscures again an essential aspect of the theory of motivation. We cannot adequately conceive of human conflict, for instance, in terms of an impersonal quantity of energy, "localized" in one part of the apparatus, the impersonal sphere of the id, and not allowed to enter another part, the sphere of the ego. Conflict and the traumatic character of conflict originate in the fact that both conflicting forces are experienced as being "mine" and are thus personalized. In this sense both belong to man as a personal being or an ego. Indeed, the very fact that the patient actually and emotionally refuses to recognize them as belonging to himself is evidence that his personality basically experiences them as "mine." The ego-character of the instinctual impulses is an essential aspect of human motivation and conflict. No doubt the Freudian way of thinking corresponds to some real cleavages in human personality, but it seems essential not to neglect the personalized unity, the ego-character, of both conflicting forces. Neurosis is not the result of a conflict between an impersonal energy, or an id, and the ego; it is rather to be considered in terms of the non-accepted or non-integrated ego-character of both conflicting forces. Personality is hurt and disturbed because both forces are "mine" and yet cannot be integrated in the same ego.[4]

The result, then, of the physical inspiration of Freud's theories is an impoverished concept of the ego, and a theory of the behaving organism

[3]Freud's attempt to make the complications of mental functioning intelligible by dissecting the function and assigning its constituents to different parts of the "apparatus" was influenced also by Fechner's remarks on dreams in his *Elemente der Psycho-physik*, as quoted by Freud himself. While avoiding the anatomical descriptions of the "Project" (1895), and describing the physical images used as pure analogies, he confesses that he can see no harm in investigating in these physical terms the way in which the mental instrument is put together. "I shall remain upon psychological ground," he says, "and I propose simply to follow the suggestion that we should picture the instrument which carries out our mental functions as resembling a compound microscope or a photographic apparatus, or something of the kind. . . . And since, at our first approach to something unknown, all we need is the assistance of provisional ideas, I shall give preference in the first instance to hypotheses of the crudest and most concrete description" (6, p. 536).

[4]The ego character of human psychological functioning as a whole was better expressed in Freud's earliest terminology, when he spoke of the *pleasure-ego* and the *reality-ego*. His master Meynert also regarded both aspects of psychological functioning as related to the ego and spoke of primary and secondary ego. Recently Fairbairn replaced the Freudian classification of personality structure by a theory in terms of a *central* ego, a *libidinal* ego, and an "internal saboteur" or *persecutory* ego. (See Fairbairn, W.R.D., *Psychoanalytic studies of the personality*. London: Tavistock Publications, 1952.)

as primarily an enclosed physical system. There is no doubt, of course, that the organic functions which underlie behaviour are, in their physical aspects, governed by physical laws. But the behavioural aspect of organismic functioning is on another level and cannot be adequately dealt with by concepts expressing only the physical characteristics.

Freud himself insisted originally on the provisional nature and crudeness of his concepts of the mental apparatus. He added that he would always be willing to drop this theoretical scaffolding (6, p. 610). But never in the course of psychoanalytical development has this actually been done. What Freud called "provisional ideas" and "hypotheses of the crudest and most concrete description" have had a permanent influence on motivation and behaviour theories.

Conclusion

As I said in the beginning, each time a science enters into a new phase of development, it requires revision of the basic concepts underlying its previous stage. Dynamic psychology, and psychology in general, should now investigate the ego, not only as a derivation of the impersonal libido, but in its own right, as the human individual's personal mode of existing and behaving. Then we must face the task of elaborating a theoretical framework which will enable us to look at the "mental apparatus" (the behaving human organism) in its fundamental cognitive openness towards the world, and at human motivation in its constructive efforts to realize tasks, projects, and a concept of the Self.

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CYCLICAL VARIATION IN STUTTERING FREQUENCY AND SOME RELATED FORMS OF VARIATION

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One of the most striking features of stuttering to both stutterer and clinician is the fluctuation in frequency of stuttering from time to time. In most cases this fluctuation appears related to the strength and character of emotions engendered by different speaking situations. For example, more stuttering is usually provoked by strangers than by familiar auditors. In many cases of stuttering another form of variation is reported upon which these situational fluctuations seem to be superimposed. This is a slow cyclical variation, from periods of frequent stuttering in most situations to periods of low frequency in most situations. The length of these periods is reported by stutterers as being regular, although varying from individual to individual from several weeks to about two months. Johnson (4), in an early account of his own stuttering, reports a cycle length of two to three weeks. Galant (1) reports periods of similar and of much greater length in stutterers whom he treated. Galant insists that cyclical variation in stuttering is directly related to cyclical fluctuations of emotional state or mood. This view would be supported by most stutterers and clinicians, although to the present writer's knowledge it has not been demonstrated.

In the light of Hersey's work (2, 3) it would seem that regular fluctuations in mood are widespread in normal individuals. The industrial employees he studied showed individually characteristic mood cycle lengths of four to nine weeks. This corresponds well with the range reported by adult stutterers for their cycles of stuttering. If, as seems likely, this cyclical variation in stuttering is tied to the dysphoric-euphoric fluctuations in emotional tone which we term "mood," the stutterers who do not report such variation of stuttering pose a problem for which several explanations are possible. They may, in fact, experience cyclical variation, but have failed to recognize it as such. They may not be subject to regular fluctuations in mood, and therefore do not show associated rhythmical fluctuation in stuttering. A third possibility is that they experience regular mood swings, but that these are for some reason not associated with variation in stuttering.

The first explanation seems unlikely in view of the adequate reports on other matters given by stuttering informants who denied cyclical variation of stuttering. Hersey's findings (2, 3) strongly suggest the generality

Canad. J. Psychol., 1956, 10 (3).

of regular mood swings, which argues against the second explanation. Hence the third explanation, a divorcement between cyclical variations in mood or affect and stuttering, appears the most probable.

The above line of reasoning led to the formulation of a hypothesis concerning individual differences in the association of affect and stuttering. It was hypothesized that a dimension of stuttering exists, one pole of which is characterized by intimate association of affect and stuttering, while the other pole is characterized by dissociation between affect and stuttering. A stutterer falling at the "dissociation" end of this continuum would be expected to show a constant frequency in stuttering, regardless of changes in his emotional state. A stutterer falling at the other extreme ("association") would be expected to show a direct relationship between frequency of stuttering and level of emotional tension.

Individuals characterized by cyclical variation in stuttering should fall toward the "association" end of the continuum and, accordingly, would be expected to show a high degree of stuttering-affect association in other respects. They would be expected to show marked variation in stuttering frequency in situations of different emotional significance. It would also be expected that, if emotional tension were reduced in a given situation, their stuttering frequency would drop markedly. Individuals not showing cyclical variation, and hence falling towards the "dissociation" extreme, would not be expected to show such variation in these other conditions. Thus it was proposed to test this dimension hypothesis by studying the relationship between three forms of variation in stuttering frequency: cyclical variation, situational variation, and adaptation variation.

Procedure

Thirty stutterers applying to the Out-Patient Department of a psychiatric hospital were studied. Six patients who were already under treatment were also included. The data were collected as part of the usual investigation procedure in admitting patients to speech therapy, except for the six patients already admitted, for whom a special research appointment was arranged. The subjects ranged in age from 17 to 44 years, with a mean age of 28 $\pm$ 7.03. Twenty-five subjects were made and five female.

Cyclical Variation

A standard series of questions was employed to determine the presence of cyclical variation of stuttering frequency. Cyclicality was recorded as present only if the subject could report the approximate length of the cycle and the gradual change from good to bad periods, and if he stated its independence from situational factors.

Although cyclicality is treated in this study as either present or absent, it should be noted that the impressions gained in the interviews suggest intermediate degrees of rhythms in stuttering. These might be demonstrated by serial observations of stuttering frequency, but they were not recorded for this study as instances of

cyclicality. Female patients presented a particular problem in that three reported periodicity in stuttering related to menstruation. Apparently immediately preceding menstruation, and for a short time after its onset, they experienced a marked increase in stuttering. The movement into and out of this period of high stuttering frequency was apparently abrupt. One of these cases also reported cyclical variation unrelated to menstruation. Only this last case was considered to show cyclical variation in stuttering as here understood.

Situational Variation

A simple test situation was used to derive an index of the degree of situational variation manifested by a particular subject. The subject was asked to read, as well as possible, a standard prose passage of 180 words; the examiner followed the reading on a scoring sheet and recorded the words which were stuttered. The frequency of stuttering in this situation was compared to the frequency of stuttering observed in spontaneous discussion in the interview. Little control was exercised over the content of spontaneous discussion, although topics suspected of being emotionally charged were avoided. By asking the subject to outline his previous treatment, his work history, or his social activities, a fairly extensive sample of spontaneous speech was obtained. The lack of standardization in this method of measuring situational variation was recognized as a serious shortcoming, but the values obtained appeared valid when related to the subjects' reports on their stuttering under various conditions.

The frequency counts from these two speaking situations were put in ratio, with the higher value (from whichever situation) as the numerator. The value 1.5, which indicates a change of one third in frequency, was arbitrarily selected as critical, and the total group was classified into "high" and "low" situational variation categories.

Some explanation should be offered for the apparent unconcern about the situation in which the highest or lowest frequency was observed. It was clear, in conducting this part of the study, that there was considerable variation in how the stutterers perceived the oral speaking situation and the interview. Some stutterers apparently took delight in reading aloud but appeared anxious in interview, while others showed the reverse tendency. It was felt that the critical matter was whether the two situations were emotionally experienced as different; in so far as this requirement was met, the methed of measuring situational variation seemed justified.

Adaptation

The prose passage already mentioned was repeated four times, making five readings in all, done at the same sitting. Other studies (**6**, **7**) indicate that the incidence of stuttering with repeated readings of the same material may be depicted by a decelerating curve of negative slope. It seems reasonable to attribute the decrease in stuttering frequency to the reduction of tension as the subject acquires familiarity with the total situation (**5**). Despite this general trend, individuals do vary in the extent to which adaptation is manifest, some showing no evidence of it. To obtain a measure of the extent of adaptation, the stuttering frequencies in the first and second readings were put in ratio, with the first frequency as numerator. The critical value of 1.5 was again used to classify the cases in "high" and "low" adaptation categories.

The index was derived from the frequencies of the first and second readings because it divided the total group into almost equal halves, and seemed at least as good as the indices obtained by using a ratio of the first reading and any other

one. When the ratio was obtained by using the first and fifth readings, for example, it increased the number of individuals in the "high" adaptation category, but did not drop from the "high" category any individuals who were "high" when the ratio was derived from the first and second readings.

Results

The three variables described were related in their dichotomized form and tested by Chi-square method for independence. The statistical method was dictated by the finding that the ratios from both the adaptation and situational variation measures showed marked positive skewness. This prevented the use of biserial or tetrachoric correlations, where the underlying assumption is that of normal distribution.

The results (Table I) indicate that the null hypothesis may be rejected at the 5 per cent level of confidence for the relationship of cyclicality of stuttering and adaptation, and for the relationship of cyclicality and situational variation. A low relationship was obtained for adaptation and situational variation, which may well be due to the crude method of measurement employed. Despite this roughness of measurement, two of the three hypothesized relationships were found, which suggests that the concept of a dimension of stuttering-affect association may be a fruitful one for further research.

The relationship of the three main variables to two other variables was investigated. It seemed possible that variation of many sorts in stuttering frequency might well be a function of the absolute level of the manifest stuttering frequency. An individual who stutters a great deal might be considered to have greater opportunity for variation than a person stuttering much less, if both are oriented towards minimizing stuttering. To determine if such a relationship existed, the frequency of stuttering in spontaneous discussion was used as a measure of the severity of stuttering. This variable was dichotomized by using the mean as the

TABLE I

Stuttering Characteristics Tested for Independence

Variables tested for independence	Chi-square	P	
Cyclical variation—adaptation	13.89	<.001	
Cyclical variation—situational variation	5.00	<.05	>.02
Adaptation—situational variation	2.27	<.20	>.10
Cyclical variation—severity	1.16		
Adaptation—severity	.54		
Situational variation—severity	.00		
Cyclical variation—age	.81		
Adaptation—age	1.22		
Situational variation—age	1.22		
Severity—age	.46		

critical value and was tested by Chi-square in relationship to the three main variables. The results (Table I) suggest that there is no relationship between severity and cyclicality, adaptation, or situational variation.

Visual inspection of the data strongly suggested a relationship between the age of the stutterer and the presence of the several forms of variation in stuttering considered here. Marked variation appeared more characteristic of the younger patients than of the older. Three out of four stutterers under 20 years of age reported cyclical variation compared to only one out of five over 35. This observation was tested by dichotomizing the subjects into two age groups with 27 years arbitrarily selected as the critical point. The results of Chi-square tests are given in Table I and indicate the relationships are not significant.

Predicting Improvement of Stuttering

In many speech therapy clinics the adaptation test is used as one of a number of tests to predict therapeutic outcome. Although there have been no reports of the statistical potency of this device in prediction, its general use suggests some value. The rationale usually employed is that those stutterers who show low adaptation demonstrate their inability to alter their expectations of stuttering associated with specific word, letter, or other cues (8). In this sense, low adaptation is considered to indicate rigidities which are important in predicting therapeutic outcome. The present study offers some justification for considering different forms of stuttering variation as different aspects of an underlying association-dissociation dimension. From this viewpoint performance on the adaptation test indicates the degree to which a person's affective experience influences his speech performance, high adaptation reflecting a close relationship between his emotions and his stuttering behaviour. For such a person one would predict some improvement in stuttering for any therapeutic process designed to reduce the amount of strong disintegrative emotionality. That is, a decrease in emotionality in any such person should be accompanied by a decrease in stuttering.

It is the general clinical impression that many forms of treatment are successful in reducing the frequency of stuttering of roughly half the stutterers treated. This may be explained by assuming that the patients showing improvement are those with a high degree of stuttering-affect association, and that they respond in speech because the treatments in one way or another do reduce violent emotional fluctuations. On the other hand, in patients with a low degree of stuttering-affect association, one would not expect a more stable emotional equilibrium to be accompanied by reductions in stuttering frequency.

A further implication would seem to be that performance on adaptation tests and the other measures of stuttering-affect association indicates

the likelihood of improvement in stuttering only up to that frequency of stuttering associated with optimum emotional status. This is a highly relative matter, for some stutterers show fluency at times of optimum emotional equilibrium, while others show a relatively high frequency. Something more than emotional stabilization is required in many stutterers to reduce the frequency of stuttering to points even near fluency. Some therapeutic approaches explicitly recognize that improvement of the stutterer's emotional attitudes towards self and others is only a first step in obtaining lasting speech improvement. In treatment programs of this sort, old habit patterns associated with stuttering are disrupted and new skills are developed in dealing with stuttering. For such therapeutic approaches it seems unlikely that adaptation performance or other measures of stuttering-affect association will be significantly related to treatment outcome.

Summary

A dimension in stutterers of the closeness of association of stuttering frequency and emotionality was proposed. This hypothesis was tested by studying the correlation of cyclical variation, adaptation, and situational variation of stuttering. Two of the three expected relationships were demonstrated. This is taken as support for the use of the dimensional concept in further study of stuttering. The implications for predicting treatment outcome are considered. It is suggested that only those stutterers with a close association between stuttering and affect are likely to show a decrease in stuttering under treatment aimed at reducing their emotional reactivity.

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Opinions and Personality. By M. Brewster Smith, Jerome S. Bruner, and Robert W. White. New York: John Wiley & Sons, Inc., 1956. (Toronto: University of Toronto Press.) Pp. vii, 294. $6.00.

Bored stiff with opinion surveys, some psychologists may pass over this little book on the basis of its title. That would be their loss, for in less than 300 pages it brings to a clear focus most of the discrete and conflicting notions about personality functioning current in our discipline. The varied approaches of social psychology, cognitive theory, psychoanalysis, and cultural anthropology, often pursued in isolation, are here finally integrated in an intelligent and convincing structure.

The bulk of the book does examine the opinions of ten men about Soviet Russia, but for Smith, Bruner and White an opinion is not a disembodied datum to be entered on a polling sheet, but a key to the life processes of a human being. "Look far enough into the origins of any opinion, and one will find . . . a sample of how the holder of that opinion copes with his world" (p. 40). With this aim the Harvard trio explored the personality of each subject in fifteen two-hour sessions. Interviews and tests were imaginatively planned, and the case studies, three of them presented in detail, are models of judicious interpretation.

All this, however, required in advance a well-articulated point of view about personality and its functioning; the development of this was the authors' real achievement, and its succinct statement in chapters 2, 3, and 10 is the theoretical core of the book. These chapters, with the Introduction and Postscript, provide an introduction to Personality Theory of a sort we have long needed. It is inclusive yet well organized, condensed but admirably clear, critical, but never doctrinaire or dogmatic.

In the tradition of H. A. Murray, the theoretical standpoint is solidly functional; indeed, the authors' central question, "What purpose is served by holding an opinion?" makes one fear that they may neglect external factors. Their statements, however, are reassuring: "Attitudes are a man's major equipment for dealing with reality . . . for balancing the demands of inner functioning and the demands of the environment" (p. 39). They do not solve the problem of how anyone can "deal with reality" while all his perceptions of it are coloured by inner strivings, but that problem is psychologically insoluble. Their standpoint here, as on other issues, is wise, mature, and modest, and their delightfully written book should be a beacon in an area where conflict and confusion have been too prevalent.

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Schools of Psychoanalytic Thought. By RUTH L. MUNROE. New York: The Dryden Press, Inc., 1955. Pp. xvi, 670. $7.50.

WRITTEN by a well-known clinical psychologist this exposition and critique of psychoanalytic schools has achieved its aim of presenting a more objective view of their tenets and divergencies than could have been achieved by an active analyst. Combining her philosophical and scientific training in psychology with an extensive experience, both personal and professional, of a variety of psychoanalytic schools, she has been able to discern many interesting theoretical trends.

For purposes of discussion and comparison she has divided the schools into two major categories. The first is the "libido" group who accept the libido theory of classical Freudianism. The second, the "non-libido" analysts, include Adler, Horney, Fromm, and Sullivan, all of whom have built up theories in rejection of this Freudian concept. Jung and Rank, who do not fit this dichotomy, are discussed separately.

The book is well organized. Each school is discussed in considerable detail under five main headings: "Terms of the Organism," "Terms of the Milieu," "The Genetic Process," "The Dynamics of the Functioning Personality," and "Pathology and Treatment." Each subsection is followed by useful summaries; and each objective presentation by the author's criticisms, comparisons and suggestions for research, printed in a different format for easy identification as "Munroe." In these comments, as well as in an introduction and epilogue, the author attempts some integration of the various approaches under what she calls a "View of Systems." This is not a new school, having no content of its own, but it offers the reader a valuable frame of reference for his own efforts at interpretation and criticism.

The author has an admitted Freudian bias and the largest section of the book is devoted to Freud and the Freudians. In her view, most of the non-libido analysts suffer from "reductionism," lack of thoroughness, and lack of attention to the biological aspects of personality. She applauds their emphasis on "self" but feels that while the Freudians have been deficient in the past they are now making good progress in ego psychology. She does not interpret Freud in any doctrinaire manner and sees no reason why most "self" theories should be considered incompatible with basic Freudian theory.

Discussion in any detail of current psychological personality theory is beyond the scope of this volume, but in the epilogue some attempt at integration is made. In general, *Schools of Psychoanalytic Thought* is an important contribution to the systematic literature on psychoanalytic concepts and practices, and provides both professional and student psychologists with a much needed one-volume survey of contemporary psychoanalysis.

M. E. ALLEN

The High School Student: a Book of Cases. By JOHN W. M. ROTHNEY. New York: Dryden Press, 1953. Pp. xii, 271. $1.90.

The Adolescent: a Book of Readings. By JEROME M. SEIDMAN (Ed.). New York: Dryden Press, 1953. Pp. xviii, 798. $4.50.

Theory and Problems of Adolescent Development. By DAVID P. AUSUBEL. New York: Grune and Stratton, 1954. Pp. xviii, 580. $11.00.

ONE of the most glaring gaps in the picket fence of clinical psychology is the literature on the adolescent. The three books listed above have been combined in one review to illustrate the attempts being made to correct this state of affairs. They represent three distinct approaches to the problem: the presentation of illustrative cases, the collection of representative studies, and the encyclopaedic textbook.

The High School Student is a volume designed primarily for teacher training. It presents 27 case studies of typical high school students, some of whom *have* problems, but none of whom were selected *because* of personality difficulties. Cases were selected randomly from 870 high school students in a 21,000 person community, representing both urban and rural areas. Data were gathered cumulatively from grades ten to twelve using such devices as questionnaires, interviews, parent and teacher conferences, tests, and behaviour descriptions. Following an introductory section describing the methods of data gathering, the cases are categorized into The Troubled Ones, The Ones in Trouble, The Happy Ones, The Physically Handicapped, and The Quiet Ones. Each section is prefaced with a series of problem questions for the reader to consider as he studies the cases: each case is concluded with discussion questions. A very brief but pointed section on the principles, problems, and methods in case studies concludes the book. This unpretentious volume makes no effort to be a text in adolescent psychology. Its purpose is to make meaningful to teachers and others the realities of teen-age thought and behaviour through organized accounts of typical students. As such it is an extremely useful teaching aid, unique in its emphasis on the normal.

In *The Adolescent*, 67 discrete studies are presented in six major sections: Adolescence, Growth and Development, Interests, Attitudes and Ideals, The Adolescent and his Peers, Multiple Group Membership, and Understanding and Helping the Adolescent. Authors represent most of the social sciences and medicine. The quality of article varies, as is usual in such collections, from deadly to delightful. Some are useful reviews of the literature, others are specific and unrelated bits of research. The majority are reprints from journals and texts. The major fault with this volume is that it is purely a collection. No effort has been made to comment on the contributions or to relate them to a central theme. The editor must have felt shy about this, for he provides a two-page table showing

the "correlation of chapters in other texts with selections in this book." This text serves one useful purpose, convenient access to a series of articles, but does little to provide a comprehensive account of development. As an adjunct to a text such as the one listed below, it is of value.

The third book, *Theory and Problems of Adolescent Development*, is quite openly a textbook in the old-fashioned sense. It was a delight to pick up a psychology text that was free from *Life*-like pictures, and to have an author tell you in the preface that he is attempting to organize the vast amount of accumulated data around a "comprehensive and integrated theory of development." Commencing with a brief but adequate historical introduction to this area, the author then discusses in Part II, the psychobiological problems of "transition"; physiological changes, physical growth and motor development with their psychological accompaniments. Part III covers the psychosocial aspects and leads into the final section on behaviour disorders, delinquency, mental hygiene, and guidance. The text is difficult to read, partly because of a heavy style, but mainly because of an excellent condensation that requires close reading attention. Reference material is good, tables of statistics have been kept to a minimum, and the use of cross-culture data is pertinent and stimulating. The author, who is both M.D. and Ph.D., combines physical and psychological data adroitly and informatively. This admirable introductory text, together with the two previous books, creates a most useful small library on the psychology of the adolescent.

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